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REPORT

OF THE

ELECTRICAL CONFERENCE

AT

PHILADELPHIA

IN

SEPTEMBER, 1884.

JANUARY 25, 1886.—Laid upon the table and ordered to be printed.

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MESSAGE

FROM THE

PRESIDENT OF THE UNITED STATES,

TRANSMITTING

Letter of the Secretary of State with report of the Electrical Conference at Philadelphia in September, 1884.

JANUARY 25, 1886.—Laid upon the table and ordered to be printed.

To the Senate and House of Representatives:

I transmit herewith a letter from the Secretary of State, which is accompanied by the report of the United States Electrical Commission of the proceedings of the National Conference of Electricians held at the city of Philadelphia in the month of September, 1884.

GROVER CLEVELAND.

EXECUTIVE MANSION,
Washington, January 25, 1886.

DEPARTMENT OF STATE,
Washington, January 25, 1886.

To the PRESIDENT:

An International Electrical Exhibition, under the auspices of the Franklin Institute of the State of Pennsylvania for the promotion of the mechanic arts, was opened in the city of Philadelphia on the 2d day of September, 1884.

Although the exhibition was organized and conducted apart from the immediate control of the United States, the advantages for useful results were distinctly recognized by Congress. The joint resolution approved February 26, 1883, granted the privilege of free entry to all articles from abroad intended and used for the purposes of the exhibition, and the act making appropriation for the sundry civil expenses of the Government, approved July 7, 1884, authorized the President to appoint a scientific commission which might, in the name of the Government, conduct a national conference of electricians in Philadelphia in the autumn of that year.

The act further provided that the commission thus appointed might invite scientific men, native and foreign, to participate in the conference, and to determine the general scope and character of its work.

In pursuance of the provisions of the act, President Arthur appointed the following distinguished scientists: Professors Henry A. Rowland, Monroe B. Snyder, George F. Barker, Simon Newcomb, C. F. Brackett, J. Willard Gibbs, Edwin J. Houston, John Trowbridge, F. C. Van Dyck and Charles A. Young, and Mr. R. A. Fisk and Dr. William H. Wahl as members of the United States Electrical Commission.

I now transmit herewith, with the view of its being transmitted to Congress, the report of the commission, together with that of its proceedings.

Respectfully submitted.

T. F. BAYARD.

(Received June 13, 1885.)

Hon. THOMAS F. BAYARD,
Secretary of State:

SIR: The undersigned have the honor of herewith submitting to you, as the report of the United States Electrical Commission, the proceedings of the National Conference of Electricians held in Philadelphia September 8–13, 1884, under the direction of said Commission appointed by his Excellency the President of the United States, to carry out the provisions of an act of Congress. We also take occasion, in presenting to you this record of the deliberations of the Conference, to add a brief statement of the duties performed by the Commission in convening the Conference.

Pursuant to a letter of instruction from Secretary Frelinghuysen, the Commission held its first meeting in Philadelphia, at the hall of the Franklin Institute, August 7, 1884, and effected the organization indicated in the accompanying memorandum. All the members of the Commission, excepting Mr. Fisk (who arrived later), were present, and participated in its deliberations.

The evident intention of the act Constituting the Commission was, that it should convene the Conference during the period when so many men interested in electrical science would naturally gather in Philadelphia. The lateness of the hour at which the Commission was constituted left, however, but a very limited margin of time for preparation. It was, therefore, necessary that prompt action be had, and, accordingly, a number of sessions of the Commission were held during the week following the first meeting.

After the preliminaries of organization and the selection of time and place of meeting for the Conference, lists of foreign and American conferees were carefully prepared, and the corresponding secretary of the Commission was instructed to send out formal invitations to all

such to be present and participate in the discussions. The lists of conferees known to have been present at the Conference are appended to this letter.

It should be remarked in this connection that the exceedingly limited time of preparation for the Conference did not allow the Commission to make out a complete list of all American electricians for invitation, nor to invite any as foreign conferees who were not expected to be in America in attendance upon the meetings of the British Association of Science or at the International Electrical Exhibition of the Franklin Institute.

A series of topics for discussion was selected by the Commission as suitable to the purposes of the Conference, and communicated to the invited Conferees. The proceedings of the Conference indicate the nature of these topics.

In addition to the meetings of the Commission August 7–12, other meetings were held September 4 and during the sessions of the Conference, in order to arrange for the deliberations of the same.

It should also be stated that it was the original intention to have conducted at the Electrical Exhibition a series of experiments under the direction of the Commission. Pending the passage of the act of Congress, the Franklin Institute had, however, through its examiners, undertaken a somewhat similar series, and it therefore did not encourage the idea that they should also be undertaken by the Commission; and this, together with the limited appropriation, led to the abandonment of the plan.

HENRY A. ROWLAND,
President of the Commission.
MONROE B. SNYDER,
Recording Secretary of the Commission.

THE NATIONAL CONFERENCE OF ELECTRICIANS.

UNITED STATES ELECTRICAL COMMISSION.

Professor Henry A. Rowland, *President*,
Johns Hopkins University, Baltimore, Md.
Professor Monroe B. Snyder, *Recording Secretary*,
High School Observatory, Philadelphia, Pa.
Professor George F. Barker, *Corresponding Secretary*,
University of Pennsylvania, Philadelphia, Pa.
Professor Simon Newcomb,
United States Nautical Almanac Office, Washington, D. C.
Dr. William H. Wahl,
Franklin Institute, Philadelphia, Pa.
} *Executive Committee.*

Professor C. F. Brackett, *College of New Jersey, Princeton, N. J.*
Mr. R. A. Fisk, *San Francisco, Cal.*
Professor J. Willard Gibbs, *Yale College, New Haven, Conn.*
Professor Edwin J. Houston, *Central High School, Philadelphia, Pa.*
Professor John Trowbridge, *Harvard College, Cambridge, Mass.*
Professor F. C. Van Dyck, *Rutgers College, New Brunswick, N. J.*
Professor Charles A. Young, *Halstead Observatory, Princeton, N. J.*

VICE-PRESIDENT.

Professor Sir William Thomson, Glasgow, Scotland.

FOREIGN CONFEREES.

W. F. Barrett, Dublin, Ireland.
Charles Carpmael, Toronto, Ontario.
J. H. Cotterill, Greenwich, England.
Conrad W. Cooke, London, England.
Thomas R. Crampton, London, England.
James Dewar, Cambridge, England.
Francis J. Fitzgerald, Dublin, Ireland.
George Forbes, London, England.
F. N. Gisborne, Ottawa, Ontario.
Oliver J. Lodge, Liverpool, England.
J. G. MacGregor, Halifax, Nova Scotia.
W. H. Preece, London, England.
A. W. Reinold, Greenwich, England.
Arthur Schuster, Manchester, England.
Silvanus P. Thompson, Bristol, England.

AMERICAN CONFEREES.

Abbe, Cleveland, Delegate from United States Signal Service, Washington, D. C.
Allen, Lieutenant James, Delegate from United States Signal Service, Washington, D. C.
Anthony, William A., Professor of Physics, Cornell University, Ithaca, N. Y.

Ayres, Brown, Professor of Physics, Louisiana University, New Orleans, La.

Barus, Dr. Carl, Assistant Geologist United States Geological Survey, 310 Dixwell avenue, New Haven, Conn.

Bell, Professor A. G., Electrician, 1500 Rhode Island avenue, Washington, D C.

Blake, E. W., Professor of Physics, Brown University, Providence, R. I.

Borden, Spencer, Manufacturer, Fall River, Mass.

Brown, R. G., Boston Synchronous Multiplex Telegraph Company, 78 Devonshire street, Boston, Mass.

Carhart, Henry S., Professor of Physics, Northwest University, Evanston, Ill.

Clarke, Charles L., Electrical Engineer, 65 Fifth avenue, New York.

Compton, A. G., Professor of Natural Philosophy, College City of New York, New York.

Cooley, LeRoy C., Professor of Physics, Vassar College, Poughkeepsie, N. Y.

Cross, Charles R., Professor of Physics, Massachusetts Institute of Technology, Boston, Mass.

DeMotte, John B., Professor of Physics, DePauw University, Greencastle, Ind.

Dolbear, A. E., Professor of Physics, Tufts College, College Hill, Mass.

Draper, Daniel, Director New York Meteorological Observatory, Central Park, New York.

Dudley, C. B., Chemist Pennsylvania Railroad Company, Altoona, Pa.

Duncan Louis, United States Navy, Electrical Exhibition, Philadelphia.

Eddy, H. T., Professor of Mechanical Engineering, University of Cincinnati, Cincinnati, Ohio.

Fiske, Bradley A., Lieutenant, Ordnance Department, U. S. A., Washington, D. C.

French, Thomas, jr., Professor of Physics, University of Cincinnati, Cincinnati, Ohio.

Fuller, George W., Electrician, Norwich, Conn.

Geyer, William E., Stevens Institute of Technology, Hoboken, N. J.

Gibbs, Wolcott, Harvard College, Cambridge, Mass.

Goodnow, A., Teacher, Baltimore, Md.

Gray, Elisha, Electrical Engineer, Highland Park, Ill.

Greely, Lieutenant A. W., Delegate from the United States Signal Service, Washington, D. C.

Griscom, W. W., President of Electro Dynamo Company, 224 Carter street, Philadelphia.

Hall, E. A., Assistant Professor of Physics, Harvard College, Cambridge, Mass.

Hamblet, James, Electrician, the Time Telegraph Company, 5 Beekman street, New York.

Hamilton, G. A., Electrician, Western Union Telegraph Company, 195 Broadway, New York.

Harkness, W. H., United States Naval Observatory, Washington, D. C.

Harrington, M. W., Professor of Astronomy and Meteorology, Ann Arbor, Mich.

Heap, D. P., Major U. S. A., Light-House Board, Washington, D. C.

Hering, Carl, Assistant Electrician International Electrical Exhibition, Philadelphia.

Hilgard, J. E., Superintendent of Coast and Geodetic Survey, Washington, D. C.

Himes, Charles F., Professor of Physics, Dickinson College, Carlisle, Pa.

Hochhausen, William, Electrician, 66 Duane street, New York.

Hodges, N. D. C., Assistant Editor "Science," Salem, Mass.

Hooper, W. L., Assistant Professor of Physics, Tufts College, College Hill, Mass.

Howell, John W., Electrician, Edison Lamp Company, East Newark, N. J.

Jewell, T. F., Lieutenant-Commander U. S. N., Topedo Station, Newport, R. I.

Keith, N. S., Secretary of Society Electrical Engineering, 237 Broadway, New York.

Kimball, A. L., Associate Professor of Physics, Johns Hopkins University, Baltimore, Md.

Kintner, C. J., Examiner in Electricity, Patent Office, Washington, D. C.
Koyl, Charles H., Maryland Hotel, Annapolis, Md.
Langley, S. P., Director of Observatory, Allegheny, Pa.
Lockwood, Thomas D., Electrician, American Bell Telephone Company, 95 Milk street, Boston, Mass.
Marks, William D., Professor of Dynamical Engineering, University of Pennsylvania, Philadelphia, Pa.
Mendenhall, T. C., Professor Physics, Ohio State University, Columbus, Ohio.
Michaelis, O. E., Captain Ordnance, U. S. A., Frankford Arsenal, Philadelphia.
Moore, J. W., Professor Physics, Lafayette College, Easton, Pa.
Murdock, J. B., Lieutenent U. S. N., Electrical Exhibition, Philadelphia.
Nichols, E. L., Professor Physics, Central University, Richmond, Ky.
Nipher, F. E., Professor Physics, Washington University, Saint Louis, Mo.
Outerbridge, A. E., Chemist, Philadelphia.
Perkins, Charles A., Assistant Professor Physics, Johns Hopkins University, Baltimore, Md.
Plush, S. M., Manager Philadelphia Local Telegraph Company, Philadelphia.
Pope, Frank L., Electrician, 32 Park Place, New York.
Rice, E. W., Electrician, Thomson-Houston Electric Company, Boston, Mass.
Ritchie, Edward S., Manufacturer, 150 Tremont street, Boston, Mass.
Rogers, W. A., Assistant Professor Astronomical Observatory, Harvard College, Cambridge, Mass.
Sampson, W. T., Commander U. S. N., Naval Observatory, Washington, D. C.
Scott, E. Alexander, Electrician, Philadelphia.
Short, Sidney H., Professor Physics, University of Denver, Denver, Colo.
Smith, T. A., Professor Mathematics and Physics, Beloit College, Beloit, Wis.
Sprague, Frank J., Electrician, 65 Fifth avenue, New York.
Tainter, Sumner, Assistant Volta Laboratory, 1221 Connecticut avenue, Washington, D. C.
Tatham, William P., President Franklin Institute, Philadelphia.
Thomson, Elihu, Electrician, Lynn, Mass.
Trowbridge, W. P., Professor Engineering, Columbia College, New York.
Upton, F. R., Electrician, Box 95, Orange, N. J.
Waldo, Dr. Leonard, Yale College Observatory, New Haven, Conn.
Wead, C. K., Professor Physics, University of Michigan, Ann Arbor, Mich.
Weston, E., Electrician, 23 Plane street, Newark, N. J.
Whitman, Frank P., Professor Physics, Rensselaer Polytechnic Institute, Troy, N. Y.

The following letter was sent by the corresponding secretary to the conferees designated by the Commission:

UNITED STATES ELECTRICAL COMMISSION,
HALL OF THE FRANKLIN INSTITUTE,
Philadelphia, Pa., August 20, 1884.

SIR: The President of the United States, in pursuance of a special provision of Congress, has appointed a scientific commission, which, in the name of the United States Government, may conduct a national conference of electricians in Philadelphia in the autumn of 1884.

This act of Congress provides "that said Commission may invite scientific men, native and foreign, to participate in the conference, and may in general determine the scope and character of its work."

I have the honor to inform you that the Commission desires you to act as a member of the National Conference of Electricians, and has instructed me to invite you to attend its meetings and take part in its deliberations and discussions.

The Commission has determined that the opening session of the Conference shall be held on the 8th of September next, at 3 o'clock p. m., in the lecture hall of the International Electrical Exhibition, and that the sessions shall be continued from day to day as may be necessary.

Your attention is respectfully called to the inclosed abstract of the proceedings of the Commission relating to the Conference.

Awaiting your reply, I remain, your obedient servant,

GEORGE F. BARKER,
Corresponding Secretary.

SESSIONS

OF THE

NATIONAL CONFERENCE OF ELECTRICIANS,

HELD AT PHILADELPHIA SEPTEMBER 8–13, 1884.

FIRST DAY.

The first session was held in the International Electrical Exhibition Building, at 3 p. m., September 8, 1884.

Professor SIMON NEWCOMB. The United States Electrical Commission has charged me with the honorable duty of calling this Conference to order and making a brief statement of its objects. It was hardly necessary to qualify this statement by the word "brief," from the very necessity of the case. It was only a short time ago—this forenoon—that I was informed of this duty. I have, therefore, had no time to prepare any remarks whatever, and will merely do what is required of me—state how it is that we come to be called together.

The enormous advance made in electricity within the past few years has resulted, as you know, in several electrical exhibitions in other countries. The object of these exhibitions is to bring together the various improvements which are made by inventors in the applications of electricity. The possibility of these improvements, I may say, parenthetically, offers a very remarkable example of the practical value of pure mathematics. It is only in consequence of the reduction of electrical phenomena to mathematical laws, and their expression in such technical terms as ohms, volts, and ampères, that it has been possible to make the great improvements with which you are so familiar in the practical applications of electricity.

The foreign exhibitions to which I have referred have generally had invited to them conferees, both from their own and from other countries, with the object of exchanging views. When this exhibition was proposed by the Franklin Institute it seemed eminently proper that the same course should be taken; that advantage should be taken of this opportunity to bring together, both from our own and from foreign countries, such experts in the science of electricity as could enlighten us on the subject of its practical application. You are brought here together as practical electricians interested in the improvements made on this

subject, and each of you desirous to learn from others. That you might so learn, however, it was necessary to have a programme of operations; and that programme has been made out in some sort under the auspices of the United States. The Franklin Institute, desiring that the electricians of the country might take advantage, as far as possible, of the opportunity thus afforded, made an application to Congress, at its last session, for assistance in the present Conference; and although in the law passed no special reference is made to the Franklin Institute Exhibition, yet we are here on the grounds of the Institute, and in some sort as their guests, and they have courteously offered to us all the facilities at their command.

We are therefore here, I say, as practical electricians, desirous to learn from each other, and exchange views on the list of subjects which has been made out by the United States Commission, with the best light it could get from the practical men with whom it consulted. We hope to have your views on all these points, and to have your support in any public measures you may deem advantageous to present to the Government, with the view of getting support for them in the future.

With this statement of the objects of the present Commission, gentlemen, I have the honor to introduce to you the president of the Commission, who has also been nominated president of the Conference, Professor Henry A. Rowland, of Johns Hopkins University, Baltimore.

ADDRESS OF PROFESSOR ROWLAND.

To the student of science who has a disposition to look into the pages of history, no life has greater interest than that of Archimedes, and yet there are few men about whom so little is known. Living more than two thousand years ago, the accounts of him which have come to us are little short of fabulous, and yet they are of such a nature that we can say without any doubt that he was a genius such as the world has seldom seen. To him we owe some of the fundamental facts of mechanics, such as the principle of the lever and the pulley, and the fact that a body immersed in a liquid loses in weight as much as an equal volume of the liquid weighs. And in military engineering his success was so great that he prolonged the siege of Syracuse by the Romans from what would probably have been a few days to three years. His engines shot against the enemy immense numbers of darts and huge stones, which mowed them down in columns, and falling on their ships destroyed them. He thrust out huge beams from the walls over the ships and drew them into the air, where they swung to and fro to the amazement and terror of the Romans and were finally dropped and sunk to the bottom of the sea. He is even said to have set them on fire by means of the reflected light of the sun. But his principal work was in geometry, and of this I only need to quote the words of Professor De Morgan referring to those geometrical works of Archimedes which have come down to us. "Here," says Professor De Morgan, "he finds all that re-

lates to the surface and solidity of the sphere, cone and cylinder and their segments. A modern work on the differential calculus would not give more results than are found here." As to the quality of the individual, the impression which his writings give us is that of a power which has never been surpassed. No one has a right to say that Newton himself, in the place of Archimedes, could have done more.

Thus before the birth of modern science, in the dim ages of the past when the light of history begins to fade and the mist of legend to cover our view, there lived a man of almost superhuman intellect whose mind seemed equally adapted to either pure or applied science. And yet Plutarch says of him, "Archimedes possessed so high a spirit, so profound a soul, and such treasures of scientific knowledge, that, though the inventions (referring to his military engines) had now obtained for him the renown of more than human sagacity, he yet would not deign to leave behind him any commentary or writing on such subjects; but, repudiating as sordid and ignoble the whole trade of engineering, and every sort of art that lends itself to mere use and profit, he placed his whole affection and ambition in those purer speculations where there can be no reference to the vulgar needs of life; studies, the superiority of which to all others is unquestioned, and in which the only doubt can be, whether the beauty and grandeur of the subjects examined, or the precision and cogency of the methods and means of proof, most deserve our admiration."

Here, then, at the dawn of science the question of the relative value of pure and applied science had been brought up. To the people of Syracuse, who had to defend themselves against an overwhelming enemy, the military engines of Archimedes were of far more interest than the whole of geometry; for, the knowledge of the ratio of the solid contents of a sphere and its circumscribed cylinder cannot bring a dead man to life or restore wealth to a plundered city. And yet, from a point of view distant more than two thousand years, we are forced to admit that Archimedes was right. Archimedes's engines of destruction have passed away, but the geometrical and mechanical truths which he discovered are to-day almost the axioms of the mathematician and the worker in physical science, and the ratio of the circumference of a circle to its radius is to-day the most important of our physical constants.

But this is only a meager part of the influence of this man. The truths which he discovered have formed a part of the education of every student of mathematics to the present time, and have given pure intellectual enjoyment to all. They have helped to form the minds of all those whom we consider great in our science, and they have done their share in that march of progress which is gradually transforming the world.

Great should be the honor in which we hold the intellect of Archimedes, but greater should be our reverence when we approach that noble spirit which could ignore all worldly considerations and prefer the truths

of geometry to the vast physical power given him by his other inventions, which were his amusements for a moment. We now see that he was right, but we cannot for a moment suppose that he foresaw, except dimly, any so-called practical advantages from his discoveries. A thousand times no! He preferred his geometrical labors because of a subtle quality of his mind, an instinct toward that which was highest and noblest and a faith that the pursuit of what is noble is the surest road to the final happiness of the individual and of the world. Our highest moral qualities are of this nature, and we despise as the lowest of the low one who is honest because "honesty is the best policy," but esteem him whose instincts lead him to honesty whatever the consequences.

So we reverence the noble and lofty spirit of Archimedes, and yet we do not at the present day quite agree with his estimate of the relative value of his works. His military inventions were far from worthy of being despised, even though the only reason were that they gave the world three more years of Archimedes's life. The world is not formed of disembodied spirits, but of men, in whom there is a wonderful combination of mind and matter, and a sound mind in a sound body is the highest type of manhood. But we also know that the mind is hampered by many considerations connected with the body. Archimedes recognized this, and his noble spirit revolted at it. But to-day we see that no progress can come from this method of treatment; the body still remains, however much we may despise it, and the buzzing of a fly can disturb the most profound thought of the philosopher.

We now study the laws of nature and seek thus to assist our bodies in obeying the thoughts of our minds. Our railroads carry us hither and thither on the earth with somewhat the facility of spirits, and our thoughts pass with almost the speed of light to the uttermost portion of the earth. The steam-engine does our work, and labor-saving machinery takes the place of our hands. With a minimum amount of labor we can to-day possess luxuries unknown even to kings in ancient times, and our minds are free to study the order of nature or engage in any intellectual pursuit we may desire. Instead of being the slaves of nature and groveling in the dust before her to find the food which we crave, we have now assumed the command, and find her a willing servant to those who know her language.

But here we reach the keystone of the problem. To command her we must know her language. Knowledge, then, is the price of her service, and she obeys not the ignorant or degraded, but grinds them into dust beneath her heel.

Knowledge, then, is power, and it is more than power; it is that which the intellect most craves and is the object of many of our highest aspirations. What truth is, is the goal of intellectual mankind in all ages, and its pursuit leads not only to intellectual but also to physical satisfaction.

The pursuit of the one leads to the other, and we shall see as we pro-

ceed that the only way for the world to progress in practical science is by the cultivation of the theoretical science.

Pure science must exist before its applications, and the truths of pure science are far more reaching in their effects than any of its applications; and yet the applications of science often have a much more immediate interest for the world at large than many discoveries in pure science, which will finally revolutionize it, both physically and mentally. They both have their importance and both are at work in causing that intellectual and material progress in which the world in now pushing forward with giant steps. But there is this difference—the names of the great inventors are seen in every paper and their deeds are recounted to the rising youth of the country as examples to be followed. And yet the discoveries of the principles on which their inventions are based may have died in comparative obscurity, with poverty knocking at the door. We are in no danger of forgetting those who have been successful in those applications of science which are in daily use, and it is useless to repeat the story of the telegraph or telephone, but it will be of more interest for me to recall to your minds a few of the landmarks in our science and then to consider the present state of our science, with a possible glance into the future.

Thus we shall obtain a clearer view of how our science has been built up and of the means which are necessary for its further progress. We shall also see the relations between pure and applied science, and the relative importance of the two in the progress of the world.

It is impossible for one here to discuss the reasons why the ancients followed their science to so short a distance and the world had to wait more than two thousand years before the light of modern science commenced to shine. It must be left to the psychologists and historians. But this I may say, modern progress is cumulative. By the study of the science of the past, the minds of men are trained for its further advance in the future, and so when there was no science to study there could be but little training of the mind in the true methods of thought.

The average intellect of mankind has improved, and what could only have been comprehended in past times by a few is to-day understood by the majority of educated persons. And this increase has been most apparent in the reason and moral sense of mankind, the two qualities of the mind which come most into play in the study of science. To the mind of the ancients, where the imagination run riot without the guide of reason or a warning from their moral sense to speak the truth, it was easier to attribute the attraction of rubbed amber to an inherent soul or essence, which, awakened by friction, went forth and brought back the small particles floating around, than to examine and find out the truth.

The simple experiment of the amber remained without investigation for 2,200 years. Had the reasoning of many modern persons been followed, we should never have had a science of electricity. Why should

anybody investigate this phenomenon, this feeble force, which could only attract a few particles of dust? The world could eat, drink, and take its ease without doing anything in the matter, and it did so for more than two thousand years of intellectual, moral, and physical degradation. Then the awakening came, and men began to feel that they were reasoning beings. They began to see that there were other pleasures in the world besides animal pleasures, and that they had been placed in this wonderful universe that they might exalt their intelligence by its proper study. No question of gain entered into the minds of these early investigators, but they were led by that instinct toward truth which indicates the highest type of man. And yet their researches have transformed the world, not only intellectually, but physically. Some would say that science had been degraded by its applications, but who that looks over the world at the present time can think so? There is no danger of this view becoming general; the danger is in the other direction, and that science shall be degraded in the estimate of the world by the idea that its principal use is to be applied to the common purposes of life. A thousand times no! Its use is in the intellectual training of mankind and the high and noble pleasure it gives to those who are born to understand it; to lift mankind above the level of the brute and to make him appreciate the beauties and wonders of nature; to cause him to stand in humiliation and awe before that universe which the intellect of ages has attempted to understand and yet has failed; to make even Newton say, "I know not what the world may think of my labors, but to myself it seems to me that I have been but as a child playing on the sea-shore; now finding some pebble rather more polished, and now some shell rather more agreeably variegated than another, while the immense ocean of truth extended itself unexplored before me."

But the great moral law of the universe here enters. If the world would only pursue those things which are high and right and noble its reward would not be confined to the minds of men. Physical rewards await it as well, and disease, that principal cause of human misery, would almost pass away when the effect of inheritance from the present generation had passed. So the pursuit of pure science brings not only the rewards I have mentioned, but the physical rewards of applied science and the pursuit of applied science gives wealth which may be again employed to further pure science. So the two react on each other to produce that perfect whole, modern science, pure and applied.

This moral law of the universe is well illustrated by the well-known story of Solomon:

> The Lord appeared to Solomon in a dream by night; and God said, Ask what I shall give thee.
>
> And Solomon said, Thou hast made thy servant king instead of David my father, and I am but a little child; I know not how to go out or come in. Give therefore thy servant an understanding heart to judge thy people, that I may discern between good and bad; for who is able to judge this thy so great a people?

And God said unto him, Because thou hast asked this thing, and hast not asked for thyself long life, neither hast asked riches for thyself, nor hast asked the life of thine enemies, but hast asked for thyself understanding to discern judgment, behold, I have done according to thy words; lo, I have given thee a wise and an understanding heart, so that there was none like thee before thee, neither after thee shall any arise like unto thee. And I have also given thee that which thou hast not asked, both riches and honor; so that there shall not be any among the kings like unto thee all thy days.

So the world, when it chose knowledge and truth above all things, acquired not only the treasures of pure theoretical science but also the wealth and riches and honor which come from applied science such as the world has never seen before and could see in no other way.

It is to William Gilbert, an English physician, that we owe the commencement of the modern science of electricity. His book on the magnet was published in 1600, and contained his electrical experiments. Thus, at this early date, the similarity of electrical to magnetic attraction was recognized. But how slowly did the subject advance! The difference between conductors and non-conductors was discovered by Grey. But not until 1746, 150 years after Gilbert, was the Leyden jar invented. Then the remarkable nature of the phenomenon became apparent, and the world was startled by it. The subtle spirit which went forth from the amber, which was so feeble as only to attract dust, now flashed forth with light and sound and heat, and could cause the strength of the giant to vanish. To the world at large there was now something worth looking into. But do we think that the spark from the Leyden jar is more wonderful than the gentle attraction of the amber? By no means; for, to the scientist, they are both equally remarkable, and beyond our powers of explanation. It is only to the vulgar and uneducated taste that the tinsel and gewgaws of an electric spark appeal more strongly than the subtle spirit of the amber. Nevertheless, despicable as the means, the spark of the Leyden jar acted as a trumpet-call to Europe and even America to come to the study of the wonderful science of electricity. At no other time has there been such excitement over any electrical discovery, and electrical experiments became general.

It was only after the discovery of the Leyden jar that the idea of an electric current occurred to mankind, and this current was even transmitted to a distance by a wire and a shock given to a person across the Thames, the water forming the return circuit. And the English experimenters even went so far as to form a circuit with the two observers two miles apart, using the earth as the return circuit. Thus the fundamental fact which forms the basis of the telegraph was early observed.

But isolated facts are of little value unless connected together by something which we call a theory, and in this line we owe much to Franklin, whose letters upon this subject appeared between 1747 and 1754. To him we owe the theory of positive and negative electricity, and the fact that they are always generated in equal amounts, a law

whose importance can scarcely be estimated. He investigated the Leyden jar, and showed that the coatings had equal positive and negative charges, and explained the fact that the jar cannot be charged when the outside coating is insulated. He invented the charge and discharge by cascade, and showed that it was the glass of the jar and not the coatings which contained the charge. He discovered the property of points in discharging an electrified body, and the identity of lightning with electricity. He also made the first experiments upon atmospheric electricity.

To Canton is due the honor of giving the first experiments of induction, but Franklin is the first who gave the general law of this species of action. Truly our country and this city should honor the memory of this man.

But it is not my purpose to repeat to you in detail the familiar history of our science. Thus far no important applications of electricity had been discovered; there was nothing but pure science to attract investigators, and thus the science remained for many years after.

But no science is complete unless it is quantitative as well as qualitative. It is now very nearly one hundred years since Coulomb laid the foundation of electrostatics and Alpinus and Cavendish commenced to lay the foundation of mathematical electricity, and they were followed by Laplace, Biot, Poisson, and Murphy.

The discoveries by Galvani and Volta in 1790 and 1800, and by Oersted in 1820, gave us the galvanic battery and electro-magnetism, and it was not until the latter date that any useful practical application was possible. Then, so complete was the science that no factor of other than minor importance was necessary to transmit intelligence from one extremity of the earth to the other.

By the labors of the immortal Faraday, electro-magnetic induction was discovered and the modern dynamo-electric machine became a certainty.

To his other labors, both experimental and theoretical, the modern science of electricity owes much, but it is familiar to all. The name of Faraday needs no eulogy from me, for it stands where it can never be hidden, and the spark which Faraday first kindled now dazzles us at every street corner. No wealth came to him, although he had only to hold out his hand for it. But the holding out of one's hand takes time which Faraday could not spare from his labors, and so the wealth which was rightly his went to others. Who will follow in his footsteps and live such a life that the thought of it almost fills one with reverence? It is not only his intellect which we admire; it is his moral qualities which fill us with awe—his noble and unselfish spirit.

The name of Faraday brings us down to modern times, whose history it is unnecessary to repeat in detail, especially as there are some now present who have contributed largely to bring the science to its present perfection.

One of the principal features which we remark in our modern science of electricity is the perfection of our means of measuring both electrical and magnetic quantities. In this connection the great names of Gauss and Weber appear, the fathers of the modern absolute system of electrical and magnetic measurement, and that of Sir William Thomson, in no degree less distinguished. On the laws of electric attraction we base our electrostatic system of measurement, and on the magnetic action of the current, the great discovery of Oersted, we base our electromagnetic system, and we connect these two systems by that great physical constant, the ratio of electro-magnetic to the electrostatic system of units.

What can be simpler in theory than the electrostatic system, based, as it is, on the law that electric attraction varies inversely as the square of the distance? We only have to know how the electricity is distributed and its attraction is known. Hence we must select the simplest possible case, such as two parallel disks, and to render the problem calculable, we add a guard ring to the movable disk. We then have the absolute electrometer of Thomson. This gives us a measure of the electric potential. Knowing the capacity and difference of potential of the surface of a condenser, we know its charge. But all these quantities, the calculation of the electrometer and the capacity of the condenser, depend upon the mathematical theory of electric distribution. Are we able to calculate the capacity of condensers of all forms? I am sorry to say we are not. The modern method of treatment is due to James Green, an English investigator, whose name should be held in honor by all electricians. But this method is what is called an inverse one. It is not a method by which we can calculate the distribution on any body at random, but the shape of the body and the electrical distribution on it are both found at once by a species, as it were, of exploration and discovery. So that we cannot make our electrometers and condensers of any shape and then calculate them, but we are forced to make them of some simple geometrical form whose solution is known. We fit our apparatus to the mathematics rather than the mathematics to the apparatus.

But when we have satisfied all the conditions we measure out our static charges as easily as a quantity of matter. The manufacturer sells the oxygen and hydrogen in iron cylinders and determines the amount by the product of the capacity of the cylinders by the pressure. Were there any buyers of electricity we might sell them a Leyden jar full and determine the amount by the product of the capacity of the jar by the electric potential. According to this analogy, then, the electricity is similar to matter and the potential fluid pressure, while the word capacity has a similar meaning in both.

In the electro-magnetic method of electrical measurement we make use of the magnetic action of the current, either on a neighboring magnet or another current or portion of the same current. The laws of the

action of a current on a magnet were discovered by Biot and Savart, and of two currents on each other by Ampère, and the results applied to practical measurement to-day give us galvanometers of all kinds and the electro-dynamometer of Weber. By the galvanometer we can measure the quantity of electricity passing at any moment, but by the electro-dynamometer we measure the integral square of the current, a quantity on which the heating of the circuit and the energy expended depends.

Thus the electro-dynamometer measures the energy from an alternating current dynamo-electric machine as easily as from one giving a continuous current, but to know this energy we must know something else besides the integral square of the current, and this is either the resistance of the circuit or the electromotive force. But the measurement of electromotive force depends on a resistence. The question then comes up as to what unit of resistance is the proper one. Here we have to refer to the mathematical theory of the subject, and the great law of the conservation of energy tells us that what is known as the absolute unit of electrical resistance is the proper one for use in this case. Hence the great practical use of determining this unit. The experiments of Kirchoff, Weber, Kohlrausch, and the British Association found a value from 1 to 3 per cent. too large.

Many years ago I myself experimented on the subject, and obtained a result about 4 per cent. too high. Recently Lord Rayleigh has taken up the matter and made a series of experiments of unparalleled accuracy in this line. The International Commission, determined on by the Electrical Congress in Paris in 1881, met in April of this year at Paris, and has now given us a legal ohm defined as being the resistance of a column of mercury 106 centimeters long and 1 millimeter in section at 0° C. The length best satisfying the experiments is about 106.25, but it was considered best to use the round number. The experiments which I have been making under an appropriation from the Government are now barely completed, but they will probably agree very well with the latter figure. Hence, we can say that we now know this unit of resistance to one part in one thousand, at least. And so we are in a position to measure the energy of a current to the same degree of accuracy, as far as this quantity is concerned.

But to measure a current by the tangent galvanometer one requires to know the intensity of the earth's magnetism, a quantity difficult to determine and constantly varying with time and place. The electro-dynamometer, when made with care, is excellent, but a good one is immensely expensive. Our methods, then, of current measurement are bad, unless carried out in a completely equipped physical laboratory. With a practical standard of electromotive force, such as a Clark's standard cell or a thermo-electric battery, this difficulty partially vanishes. Better, perhaps, we might make simple electro-dynamometers with constants determined by comparisons with a more costly instrument.

But where shall these standards be kept? Evidently the Government, which decides on our standards of weights and measures, should take in charge the electrical standards, and possibly also the thermometric standards. The formation of such a Bureau of Physical Standards will be brought to the attention of this Conference.

Having given certain standards then, the measurement of currents and current energy becomes easy. The amount of heat generated in a wire of known resistance by a known current is also easily found from the absolute system of electrical measurement.

Besides the two so-called absolute systems of measurement of electricity and electric currents, we have also one based on the chemical action of the current whose laws were discovered by Faraday. Knowing the electro-chemical equivalent of some substance, we are able to measure the time integral of the current or the total quantity of the current which has passed.

The absolute measurement of magnetism is equally simple with that of electricity, and it is a common observation to find the earth's magnetic force. But Faraday has put in our hands a very simple method of measuring a magnetic field, and to-day all are familiar with his beautiful laws with respect to magnetic lines of force. We know the laws of electro-magnetism, and just how many lines of force (better induction) can pass through a piece of iron of given cross-section, and what is their relative resistance when passing through air or iron. In fact, we have all that is necessary for a complete theory of the dynamo-electric machine, and consequently we find that the latter agrees perfectly with theory, and no fact has been observed with reference to it which could not have been foreseen from theory by a person of proper intelligence.

This part of electrical science, the measurement of electrical and magnetic quantities, is thus in a very forward state, based, as it is, on the mathematical theory of the subject. But, in reality, this forms but a very small portion of our science. Shall we be contented with a simple measurement of what we know nothing? I think nobody would care to stop at this point, although he might be forced to do so. The mind of man is of a nobler cast, and seeks knowledge for itself alone. We are not so base as to be honest because "Honesty is the best policy," neither are we so ignoble as to seek knowledge because "Knowledge is power"—two sayings which are certainly true but low and sordid in their tone.

We have, then, the beautiful fabric of mathematical electricity given to the world by Poisson, Green, Helmholtz, Thomson, Maxwell, and others whose names are immortal. No hypothesis as to the nature of electricity rests at its base. Starting from the most simple laws of electricity and magnetism, it rises from a stable foundation and rears its form high in the air, never to be overturned, whatever the fate of the so-called electric fluid or the ultimate theory of magnetism. On the simple fact that there is no electric force inside a closed conductor, it is proved that the electric attraction and repulsion varies inversely as the

square of the distance. The fact is sufficient to give us the whole theory of electrostatic distribution on conductors.

From the simple fact that we can break a magnet up into parts which are similar to each other, and that these parts attract and repel each other in a certain manner, we derive many important facts with regard to magnetism.

From the magnetic action of the current we find, by an application of the great law of conservation of energy, all the laws of induced currents, either from magnets or other currents. By an almost superhuman effort of the intellect we detach our electric currents from matter, and suppose them to take place in the ether of space, and we have the grand electro-magnetic theory of light given to us by Maxwell.

But the subject is too vast to be treated in a moment. Suffice it to say that no person at the present day has the right to express an opinion on any theoretical question connected with electricity without a knowledge of its mathematics.

This study has led us to alter our ideas on many questions. What is the mechanism of electric or magnetic attraction? Faraday has given us his idea of lines of force, and has made them play an important part in the theory of magnetic induction. When treated mathematically, Maxwell has shown that all electric and magnetic attractions can be explained by a tension along the lines of force and pressure at right angles to them—an idea due to Faraday.

The mathematical theory of these lines shows that all electrostatic forces between either conductors or non-conductors can be explained in this manner. As the laws of magnetic attraction are the same in every way as electrostatic attraction, if we should do away with electric conduction, it follows that magnetic attraction is to be explained in exactly the same manner. In obtaining this result Maxwell calculated the forces acting on the medium at every point, and compares these with imaginary stresses in a medium at the given point. Hence, the energy stored up can be represented either as due to the mutual attraction of the electricity at a distance, or to the stresses in the medium at every point, and thus, as Thomson has shown, by a volume integral of the square of the force at every point. Hence, we are at liberty to deny the existence of all action at a distance, and attribute it to the intervening medium, which, to be logical, we must assume to be continuous and not molecular in constitution.

Thomson has pointed out that magnetism must be of the nature of rotation, such as possibly vortex motion in a fluid, and Maxwell has done something toward making a mechanical model of such a medium. Thomson's wonderful address at Montreal has also given us much to think of in the same direction.

But here we have reached the limit of our science, and even that servant of our reason, imagination, fails us. We are yet unable to picture to ourselves what takes place in a medium subject to electrostatic ac-

tion. We are face to face with the great problem of nature, and the questions, what is matter? what is electricity? evoke no answer from the wisest among us. Our mathematics has guided us safely up to a certain point and will guide us still further; science will advance and we shall know more. But, for the present, this is the limit which we have yet attained in this direction. However, the idea of a medium is still serviceable in other portions of our science.

We have seen that the medium explains the electrical and magnetic attraction of bodies at rest. The question then comes up as to what happens in the medium when these bodies moves. Are the imaginary stresses in the medium transmitted from place to place instantaneously or do they require time? Mathematics in the hands of the immortal Maxwell has answered this question, and we now know that any magnetic or electric disturbance is propagated through space with a velocity equal to the ratio of the electro-magnetic to the electrostatic unit of electricity. This great physical constant has now been found by experiment to be equal to the velocity of light, and thus has arisen that great modern theory, Maxwell's electro-magnetic theory of light. Indeed, at the present day, so perfectly does this theory agree with experiment that we can almost regard it as a certainty. The velocity of light and the ratio of the units agree far within the limits of experimental error. The fact that bodies having a true (not electrolytic) electric conduction are always more or less opaque, the refraction and dispersion of light, double refraction, and diffraction, all are explained on this theory with an ease and simplicity wanting in all other theories; and, lastly, an electro-magnetic phenomenon has been discovered, which, when applied to this theory of light, explains the rotation of the plane of polarization produced by a magnet. There is no fact in nature seriously in disagreement with this theory, and it serves to connect two of our most important branches of physics, light and electricity.

But some physicists say that it is not a true theory, because it is not mechanical, the object of these physicists being to reduce every phenomenon of nature to matter and motion. Whether this is necessary or not I leave to the philosophers. But it is to be noted that the old mechanical theory that light is a vibration in a medium having the properties of an elastic solid is not entirely at variance with the new theory. The medium we call ether. The electro-magnetic theory says that the waves of light are waves of electric displacement, while the old theory says they are waves of ether. Make electricity and the ether equal to each other and the two theories become one. We have arrived at that hazy and unsatisfactory theory of Edlund that ether and electricity are one, except that by this theory electricity is presented to us as an elastic solid!

But the ground trembles beneath us, and we shall soon be plunged in the mire of vague speculation if we do not draw back.

Among the other questions which depend for their solution on the

presence of a medium may be mentioned the mutual action of two electrified bodies moving in space. It has been found that electricity carried through space on a charged body has exactly the same magnetic effect on a stationary magnetic needle as if it had been conducted.

But when electrified bodies move uniformly forward in space, we can conceive of no mutual effect from such motion unless it is relative to a medium, for we cannot even conceive of absolute motion.

Assuming the medium to exist, we then know that a positively and negatively charged body flying through space with the velocity of light would have their electric attraction just balanced by their magnetic repulsion, and so would exert no force on each other.

But it is a most wonderful fact that we have never been able to discover anything on the earth by which our motion through a medium can be directly proved. Carried, as we suppose, by the earth with immense velocity through regions of space filled with ether, we have never yet been able to prove any direct influence from this etherial wind.

The assumption of a medium allows us to solve in some cases that problem so long under discussion by electricians—namely, the true velocity of an electric current. We now know that the term velocity hardly applies to this case, and that the current arrives at different points so gradually that we know not when to say it has arrived. But there is certainly a minimum time when even an infinitesimal current can reach a distant point. Suppose two wires stretched in space with their ends near together at one end and a Leyden jar be discharged from one to the other at the near end. The *minimum* possible time of obtaining a spark at the distant end will evidently be the time required by light to pass from the Leyden jar to the distant point, not around the wire, but in a straight line. In this case the greatest maximum velocity is thus twice that of light reckoned around the wire, and may be any amount greater when we bend the wire. For all ordinary distances this velocity may be considered infinite, and the retardation to depend only on the electrostatic capacity and magnetic self-induction of the wire. Treated in this way, we have Thomson's mathematical theory of the propagation of an electric wave along a telegraph wire or cable, a theory of great practical use in telegraphy and telephony. But until the action in the external medium is also taken into account, it can only be considered an approximation. For we can never move a magnet, discharge a Leyden jar, or complete the circuit of a battery, without causing a wave of electro-magnetic disturbance in the ether, and every signal which is sent along a telegraph line is accompanied by a wave in the ether, which travels outward into space with the velocity of light. Truly the idea of a medium is to-day the keystone of electrical theory, but we can hardly suppose that it has even yet attained a fraction of the importance to which it is destined to rise.

Let me now call your attention to one of the most wonderful facts connected with electrical science. When we are dealing with the elec-

trostatic action of electricity, we find that it is the so-called electric fluid which attracts the opposite. Not only do we observe the attraction of bodies oppositely charged, but the electricity itself on the two bodies is displaced by its mutual action. But when we come to investigate the mutual attraction or repulsion of electric currents on each other, we find an entirely different law. In this case the conductors carrying the currents attract or repel each other, but the currents within those conductors have no influence of attraction or repulsion to displace themselves within the body of the conductor. In other words, the current is not displaced by the action of a neighboring magnet, but flows on calmly as if it were not present.

This to me is one of the most wonderful facts in electrical science, and lies at the foundation of our science. It cannot be ignored in any further progress we may make in electrical theory, but points out a radical difference between electrostatic and electro-magnetic action.

I have said there is no action of a magnet in displacing an electric current, and have thus stated the broad general fact, and which is perfectly true in some metals. But in others there is a small action which changes in direction with the material. The elements of the electric current within the material are rotated around the lines of magnetic force, sometimes in one direction and sometimes in the other, according to the material. But the action is, in all cases, very weak. When applied to the electro-magnetic theory of light, this action leads to the magnetic rotation of the plane of polarization of light. As to the explanation of both these actions, Thomson has remarked in the case of light, from dynamical considerations, the rotation can only come from a true rotation of something in the magnetic field, and leads us to think of all magnetic action as of the nature of vortex motion in a fluid. But here our theory ends for the present. We have obtained a clew but it is not yet worked up.

I have now taken a rapid glance at some of the modern advances of electrical science, and we have not yet had to give up the old idea that electricity is liquid. To the profound thinker this idea is very vague, and there are some facts at variance with it, but it is still useful. We often hear persons say that this old idea is gone, and that electricity is "force," whatever they may mean by that. But let us see. The work or energy of an electric current between any two points is the quantity of electricity passed multiplied by the potential; this work goes to heating the wire. Let a current of water be passing in a pipe, and the quantity of water multiplied by the difference of pressure between two points gives us the work which has been done in the intervening space, and which has produced heat. The analogy is complete. No electricity has been destroyed in the one case, or water in the other, but the work has come from the fall of potential in the one case, and the fall of pressure in the other; the resultant is the same in both—heat. Again, we can obtain work from the mutual attraction and repulsion of elec-

trified bodies, and the work in this case always comes from the change of potential between the bodies while the electric charges remain undisturbed in quantity. Electricity, then, is not energy, but is more of the nature of matter.

So far for electricity in the state of rest or steady flow. But when it changes from rest to motion, all known liquids have a property known as inertia; furthermore, they have weight. But the electric fluid has neither inertia nor weight as far as we have yet experimented, and in this respect differs from all known matter. Furthermore, we have never yet been able to separate electricity from ordinary matter. When we pass electricity through a vacuum, the resistance becomes less and less, and one may have hopes of finally having an electric current through a vacuum. But, as the exhaustion proceeds, we observe that the resistance begins to increase until it reaches such a point that no discharge can take place. Electricity cannot exist, then, without matter, a fact fatal to the idea of a fluid, however useful that may be. We have but one conclusion from this, and that is that *electricity is a property of matter.* Do with it what we may, it can never be separated from matter, and when we have an electrical separation the lines of force must always begin and end in matter.

The theory of matter, then, includes electricity and magnetism, and hence light; it includes gravitation, heat, and chemical action; it forms the great problem of the universe. When we know what matter is, then the theories of light and heat will also be perfect; then, and only then, shall we know what is electricity and what is magnetism.

It is the problem of the universe which *looms* up before us and before which we stand in awe. The intellect of the greatest among us appears but feeble, and we all, like Newton, appear but as children on the seashore. But how few of us find the shells which Newton did, and how few of us try. The problem is vast and the means for its solution must be of corresponding magnitude. Our progress so far has been but small. When we push our inquiry in any direction we soon reach a limit; the region of the unknown is infinitely greater than the known, and there is no fear of there not being work for the whole world for centuries to come. As to the practical applications which await us, the telegraph, the telephone, and electric lighting are but child's play to what the world will see in the future.

But what is necessary to attain these results? We have seen how the feeble spirit, which was waked up by friction in the amber and went forth to draw in light bodies, has grown until it now dazzles the world by its brilliancy, and carries our thoughts from one extremity of the world to the other. It is the genius of Aladdin's lamp which, when thoroughly roused, goes forth into the world to do us service, and returns bearing us wealth and honor and riches. But it can never be the servant of an ignorant or lazy world. Like the genius of Aladdin's lamp it appeared to the world when the amber was rubbed, but the world knew not the

language in which to give it orders, and was too lazy to learn it. The spirit of the amber appeared before them to receive its orders, but was only gazed at in silly wonder, and retired in disgust. They had but to order it and it would have gone to the uttermost part of the earth with almost the velocity of light to do their bidding. But in their ignorance they knew not its language. For two thousand years they did not study it, and when they then began to do so, it took them two hundred and fifty years to learn the language sufficiently to make a messenger of it. And even now we are but children studying its A B C. It is knowledge, more knowledge, that we want.

I have briefly recounted the advances which we have now made in one science, and, however beautiful it may appear, we have soon reached the limit of the known, and have stood in wonder before the vast unknown. For very much of our science we see no practical applications, but we value it no less on that account. We study it because we have been gifted with minds whose exercise delights us, and because it seems to us one of the highest and noblest of employments. And we know by the history of the past that the progress of the world depends on our pursuit, and that practical applications, such as the world has never even conceived of, await us. It is necessary that some should go before to clear the way for the world's advance.

This is the work of the pure scientist; to him the problem of the universe is worth devoting his life, and he looks upon wealth as only adding to his means of research. He hopes not to solve the problem himself, but is contented if he may add some small portion to human knowledge; if he may but do his part in the march of human progress. He looks not for practical applications, but he knows full well that his most abstruse discoveries will finally be made useful to mankind at large, and so troubles himself no further about it.

The science which he creates is studied by others. Their minds are educated by it and their hearts entranced by its beauties. And some are led to devote their lives to its further advancement. But the whole world benefits by it intellectually. The wayward spirit of the amber has vanished forever, and prosaic, law-abiding electricity has taken its place even in the estimation of the most ignorant. The world has advanced, and in great part from the study of science.

Then comes the practical man, who sees that other benefits can be reaped besides those of pure intellectual enjoyment. While the investigator toils to understand the problem of the universe, the practical man seeks to make a servant of our knowledge. He seeks to increase the power of our bodies and to make the bonds by which the mind is united to it less irksome. It is he that increases the wealth of the world, and thus allows those so disposed to cultivate their tastes and to elevate themselves above the savages. The progress of the world depends upon his inventions.

Let not, then, the devotee of pure science despise practical science,

nor the inventor look upon the scientific discoverer as a mere visionary person. They are both necessary to the world's progress, and they are necessary to each other.

To-day our country, by its liberal patent laws, encourages applied science. We point to our inventions with pride, and our machinery in many of the arts is not surpassed. But in the cultivation of the pure sciences we are but children in the eyes of the world. Our country has now attained wealth, and this wealth should partly go in this direction. We have attained an honorable position in applied science, and now let us give back to the world what we have received in the shape of pure science. Thus shall we no longer be dependent, but shall earn our own science as well as inventions.

Let physical laboratories arise; let men of genius be placed at their head, and, best of all, let them be encouraged to pursue their work by the sympathy of those around them. Let the professors be given a liberal salary, so that men of talent may be contented. Let technical schools also be founded, and let them train men to carry forward the great work of applied science.

Let them not be machines to grind out graduates by the thousand, irrespective of quality. But let each one be trained in theoretical science, leaving most of his practical science to be learned afterward, avoiding, however, overtraining. Life is too short for one man to know everything, but it is not too short to know more than is taught in most of our technical schools. It is not telegraph operators but electrical engineers that the future demands.

Such a day has almost come to our country and we welcome its approach.

Then and not till then should our country be proud and point with satisfaction to her discoveries in science, pure and applied, while she has knowledge enough to stand in humiliation before that great undiscovered ocean of truth on whose shores Newton thought he had but played.

ADDRESS OF SIR WILLIAM THOMSON.

In the beautiful address which has been delivered we have an admirable exposition of the relations between pure science and the applications of science to the wants of mankind. We have had a beautiful vindication of the independence and dignity of pure science, and we have had a most liberal and intelligent exposition of the action and reaction of those two great forces continually having effect in respect to the relations between pure and applied science.

In the illustrations which we have heard with so much interest from the life and doings of Archimedes, from the sayings and discoveries of Newton, and from the model life of a philosopher presented to us by Faraday, we have seen how, in every case, science for its own sake must be the great object; how love of truth and the discovery of truth must be the reward, the ambition, and the solace of the investigator. But we

have also seen how closely the applications of science are related to scientific discovery. Archimedes despised his warlike engines. I doubt not that if we could see into the heart of hearts of that great man, we should find him regretting that science should be applied in any way in the art of destruction, even though that art of destruction was for the noble application to self-defense against an aggressor. I doubt not but that Archimedes, in his mind, looked forward to a better time when science should be applied, and great engines should be devised, for other purposes than that of infliction of wounds and death even on the enemies of your beloved country. In his own mind, Archimedes perhaps scarcely recognized how much benefit his pure science derived from the necessity under which he felt himself to apply it to practical purposes. He could not go through his great work of devising those warlike engines for throwing stones and battering down walls without investigating and finding out the laws of dynamics. He could not invent his great burning machine without studying the laws of optics, and how much of his own science, that he loved more than anything else, came from the necessity of finding ways and means of doing that which was necessary for himself and his fellow countrymen in their great distress, I think probably he scarcely recognized in his own mind.

Newton, again, passed the last half of his life as an engineer, virtually, in the service of the state as master of the mint. Nothing is to be so deeply regretted in Newton's career as that the last part of his life was so absorbed in his work for the public service that he was prevented from carrying out some of his great investigations. His sense of duty was so strong, his conscientiousness so much a part of himself, that he would not listen to any attempts on the part of those interested in science to call him back to purely scientific investigations. The work must be done. He must vindicate the credit of the nation in issuing good coins which should be 100 per cent. of their face value. In his great effort he was seconded by another man of transcendentally philosophic character, Locke, who ultimately led the British Government to prohibit the use of clipped coins, or anything less than its face value, and at a great national sacrifice to call in all the coin which was under weight, and issue coin of true value for it.

The work of a great mathematician and a great philosopher, applied to a matter of abstract justice, is here purely and interestingly exemplified. But alas for science that Newton should have been master of the mint, and only master of the mint, for the last years of his life. We have had, indeed, some evidence, new to the world, brought recently before the British Association at Montreal, to the effect that Newton really had done many things that do not appear in his "Principia," or in his "Optics," or in his "Universal Arithmetic," or in any of his works hitherto published, and that a sense of public duty alone prevented him, not only from going on and investigating more, but even kept him from publishing that which he had done, and which remains in manuscript to this day, proving that he was far ahead of anything that was then

known in respect to some of the principles in physical astronomy and optics that have been worked out since his time by persons following in his steps.

I say that this Electrical Conference, of which this is the first meeting, is to be viewed as a piece of liberal progress of the nineteenth century of the great nations of Europe and America, according to which science and its practical applications go hand in hand and mutually act and interact beneficially. If there had been an electrical conference, an international bureau of weights and measures, an international society of astronomy in Newton's time, all that Newton did for his countrymen in the way of improving the coinage and improving the mechanics and engineering of the mint would have been done, and a great deal that he left undone, and a great deal that his powers and genius allowed him to do, might have been done, and we might have had the fruits of that to this time.

An electrical conference, such as we are now entering upon, with the objects that have been put before us by Professors Newcomb and Rowland, is of the most superlative use, not merely for the practical purposes for which it is designed, but for the recognition of applications to practical purposes, which all men who cultivate science must feel to be most potent in guiding their own work, even in the most transcendental regions of thought. The special objects of this international conferencey are partl carrying out and partly expanding those which have been put forward by a committee of the British Association as long ago, I think, as 1862, and then again by the Conference of Electricians in Paris, which was held in the years 1881–'82–'83.

The introduction of an international system of electrical measurement, as it has been spoken of by Professor Rowland, is one of the most important subjects that will come under discussion of this conference.

Another very, very important object is the laying out of international standards. That is a matter of the most superlative importance for the scientific worker as well as for the engineer. It is as necessary that we should be able to measure electricity as it is that we should be able to weigh out quantities of tea and sugar, fish, meat, &c. We have, for example, a spring balance for weighing fish, a gigantic balance for weighing a loaded truck of 7 or 10 tons. Those are the basis of weighing and measuring masses by gravitation, which are well carried out in ordinary life. Quite corresponding aids, only through a wider range, are wanted for electricity. The smallest weights that the chemist needs amount to one-tenth to one-hundredth of a milligram; the gram is a thousand times as large, and a ton is one hundred million times as large. In ordinary weighing, we want to weigh from a very small quantity, say one-hundredth of a milligram, to one hundred million times as much. We want to do the same thing in electricity. The vast and intricate developments of electrical science on the one hand show us the thousandth or hundred thousandth of an ampere, as in the splendid experiments which have been made on solar radiation by Langley with his bolometer, and on

the other hand five thousand amperes. Rowland's investigations on the magnitude of the ohm help to give us the foundation to bridge over the absolute measurement of currents, from those of Langley's, which require the determination of the hundred thousandth or the millionth of an ampere, to the current of an incandescent lamp, such as an Edison lamp of six-tenths of an ampere, and to the current of five thousand amperes. When we have a central station, such as that which is so admirably worked out by Edison in New York, and which will be worked out more and more in all cities of the world, by which one large station shall give light to every place within a quarter of a mile of it, the wants of practical measurement for electricity will correspond quite in magnitude to the wants of ordinary weighing.

We want not only instruments that shall be as convenient as the fish-wife's spring balance for weighing fish or the automatic balance for weighing a railway truck, but we want quick, accurate instruments, prepared from absolute standards. As has been stated by Professor Rowland, the comparison of practical instruments with absolute standards is one requiring the superrefinement of scientific methods and the hardest work of the scientific investigator in his laboratory. Lord Rayleigh has done this to a remarkable degree. He has produced an absolute electro-dynamometer free from the objections that Professor Rowland has spoken of with regard to the difficulty of measuring the continually varying wave of terrestrial and magnetic force. This instrument is now in the Cavendish laboratory, in Cambridge, England. The comparison of this with other instruments requires the difficult transportation of instruments, or the very difficult method of electro-chemical measurement which has been described to us.

The great object of an international conference must be the laying out of convenient standards—a convenient, absolute milli-ampere-meter, and a convenient, absolute hecto-ampere-meter which will measure a few hundred amperes in a convenient way. The establishment of such instruments in all the cities of the United States and Europe is one of the first objects of the Electrical Conference. We must not hope too much from this one conference. If it opens the way to carrying out such an object, those who have worked as some have done for the public good in the way of promoting the institution of this conference will have their reward.

I fear I have taxed your patience too long; but I cannot sit down without again reflecting on the men whose lives have been patterns to the world. There is none more remarkable perhaps than the man of this country, Joseph Henry, who ended his days here. He and Faraday were patterns of scientific investigators. In some degree they went parallel and made similar scientific discoveries. Henry, indeed, preceded Faraday in the great discovery of the electro-magnetic induction between unmoved conductors. Henry gave the warmest welcome to all practical applications of his discoveries. He sought to make none himself, not because he superciliously despised the applications of science

to the public good, but because his own convictions constrained him to go on in pure science; because he felt, as Professor Rowland has said so well in respect to Faraday, that it would have taken him from his work to have devoted himself at all to the practical applications of his discoveries. But what a beautiful trait of character it is to see what a kindly welcome he gave to those who did make the practical applications. He saw what might be done, but deliberately left it to others.

I know no other parallel to Henry's action in this respect except that of Faraday himself. The beautiful and simple manner in which Faraday describes the dynamo, which is making such a mechanical and engineering revolution in the world, is remarkable: "Put a little more wire together, make the thing a little larger, and there is a vast power there, competent to do great things." I wish I could remember his words. He says in the very simplest manner that he must leave this to others. "I have to go on; I have something before me that I must do. This will be done. This will come perhaps, but not by me." I have only been able to clumsily repeat the idea in ten times the number of words, as I remember it, from reading his paper a long time ago. He thought little of the practical application; but it was not one of those gifts given with an off-hand generosity because the giver did not value it. Faraday valued the practical application. He was, like Newton, in the public service as an engineer. He was scientific adviser to the English Board of Light-Houses, as Henry was scientific adviser to the United States Board of Light-Houses.

The friendly welcome that Faraday gave to the dynamo is quite refreshing to think of. When he was first shown a great dynamo with power enough to produce the electric light he said: "I gave it to you as an infant and you bring forth the child as a giant." No one took a greater pleasure in the application of his science than Faraday. He was content to stand by and see the great results that were obtained, while he remained at work in the Royal Institution to the very end of his working days, trying to see deeper and deeper into the properties of matter, and trying to penetrate further along that line of investigation you have heard described so admirably by Professor Rowland, the relation between electro-magnetism and gravitation.

I must say, in conclusion, that I greatly wish success to the Electrical Conference in its twofold object of promoting the useful applications of science to the ordinary wants of man, and, on the other hand, to give tone and character and vigor, in one respect, to the purely scientific investigator, and to give him means of observing and seeing, to bring forward to him suggestions which the most ardent student in the laboratory or the mathematician in his retirement could scarcely arrive at without observations of the phenomena which are brought before him in these great applications of science, such as we see now in electric lighting and electric transmission of force by the dynamo electric machine.

The Conference then adjourned to 3 p. m. on the 9th.

SECOND DAY.

The Conference was called to order on Tuesday, 9th instant, by the president, Professor Henry A. Rowland, who introduced Professor Abbe to speak on the work of the United States Signal Service in relation to atmospheric electricity and earth currents.

Professor ABBE. Before I speak of the actual work going on in the United States Signal Service, I have the following letter to read at the request of the Chief Signal Officer. Myself and confreres have the honor to lay before you the inclosed documents and copies of letters recently received by him through the State Department relative to the simultaneous use of the same wire for telegraph and telephone communication. These documents not belonging to our business, we have, with the consent of the Chief Signal Officer, asked that they may be referred to the Conference, to be referred by it to a committee, to be reported upon at the proper time:

PHILADELPHIA, *September* 8, 1884.

Professor H. A. ROWLAND,

President United States National Electrical Conference:

SIR: At the request of the Chief Signal Officer, General W. B. Hazen, we have the honor to lay before you the inclosed documents, being copies of letters recently received by him through the State Department relative to the simultaneous use of the same wire for telegraphic and telephonic communication. General Hazen would be pleased to have the views and the experience of the electricians of this country as to the general practicability of Mr. C. Langdon Davies's or any other scheme for this double use of the same wire.

Hoping that this communication will be considered worthy of a report by a committee of experts in telegraphy and electricity, we remain, very respectfully, yours,

CLEVELAND ABBE,

Delegate from the Chief Signal Office, U. S. A.

DEPARTMENT OF STATE,

Washington, June 3, 1884.

The Hon. ROBERT T. LINCOLN,

Secretary of War:

SIR: With reference to the inclosed copy of a dispatch to this Department concerning the International Electrical Conference, held at Paris in April last, I have the honor to call your attention to resolution 2d as to the electrical currents and lightning conductors, and to inquire whether the Signal Office of your Department can take charge of the annual observations requested by the Conference.

I have the honor to be, sir, your obedient servant,

FRED'K T. FRELINGHUYSEN.

Inclosure: Mr. Morton to Mr. Frelinghuysen, No. 554, May 5, 1884. Copy.

A true copy.

LOUIS V. CAZIARC,

First Lieutenant, Second Artillery, Acting Signal Officer, June 10, 1884.

TELEPHONE-CUM-TELEGRAPH.

110 CAMERON STREET,
London, July 17, 1884.

SIR: My recent researches have resulted in the production of a system of simultaneous telephony and telegraphy, which is free from the grave disadvantages associated with other attempts to accomplish the same object.

My telephone-cum-telegraph does not require the graduation of the telegraph currents, the introduction of resistances into the wires, the increase of the batteries, or any other interference with the telegraphic installation as it now stands. Not only is it unnecessary to introduce such impediments into every wire in the vicinity carrying electric currents, but I do not even introduce them into the wires I use for the telephone.

Without modifying or interfering with the telegraph wires, batteries, currents, instruments, or accessories, perfectly indifferent to any number of neighboring wires employed in the transmission of electric currents (whose induction I shut out of my instruments), I have, in an experiment of six weeks' duration, held clear and distinct communication by telephone between London and another place in England, *on the existing telegraph wires*, simultaneously with the passage on the same wires, and on numerous other wires attached to the same posts, of telegrams transmitted by means of numerous forms of telegraph instruments, from the simplest to the most complex, including even Wheatstone automatic machines working at a speed of 240 words a minute.

I am now ready to make experimental demonstration of any system on the wires of telegraph administrations interested in adding telephony to all the services which the existing telegraph wires are capable of rendering

I am, sir, your obedient servant,

C. LANGDON DAVIES.

LEGATION OF THE UNITED STATES,
Brussels, July 20, 1884.

SIR: Referring to my No. 66, of December 8, 1882, I have the honor to transmit herewith a printed letter received from Mr. C. Langdon Davies, of London, from which it appears that he claims to be able to hold clear and distinct communications by telephone between London and another place in England, on the district telegraph wires, simultaneously with the passage on the same wires, and on numerous other wires attached to the same post, of telegrams transmitted by numerous forms of telegraph instruments, from the most simple to the most complex.

Believing that this may be of interest to the Secretary of War, I have the honor to bring it to your notice, and to be, sir,

Your obedient servant,

NICHOLAS FISH.

Hon. FREDERICK T. FRELINGHUYSEN.

Inclosure: Mr. C. Langdon Davies to Mr. Fish, 17th July, 1884.

DEPARTMENT OF STATE,
Washington, August 12, 1884.

The Hon. ROBERT LINCOLN,
Secretary of War:

SIR: Referring to the letter of your Department of August 3, 1882, I have the honor to inclose a copy of dispatch No. 245 from our minister at Brussels in regard to telephonic experiments on the ordinary telegraph wires.

I have the honor to be, sir, your obedient servant,

JOHN DAVIS,
Acting Secretary.

Inclosure: Mr. Fish to Mr. Frelinghuysen, July 20, 1884.

On behalf of the Chief Signal Officer of the Army, we have the honor to bring before you a few questions relating to the work which has been done, and the possible future work in atmospheric electricity and ground current throughout this country. I will insert a preface to portions of this by a short historical statement of the manner in which work has grown on our hands.

You will recall that a few years ago in this country we knew nothing of atmospheric electricity from actual observations that had been published in any scientific journal, except perhaps those of Wisclicrauns, at Saint Louis, and J. D. Everett, at Halifax, in Nova Scotia. Both these gentlemen had, with good apparatus, made a series of observations on atmospheric electricity. Other than their work, the record of the actual accurate work of this country is almost a blank.

Some years ago the committee of the American Medical Association urged that the Weather Bureau take up this matter, but we replied to them that it was a medical question; that it interested us very much, but that we preferred to wait until the electricians moved in the matter. On the other hand, it became necessary, in 1880–'81, to instruct a few observers in atmospheric electricity for observations to be made in the Arctic regions in connection with the international Arctic expeditions. We also, in 1881, began the instruction of two observers under the special tuition of Professors Trowbridge and Rowland, at Harvard and Johns Hopkins, with the view to the training of our observers in accurate, systematic observations. Of the actual results thus far obtained by these two men, I need only say that photographic records are now being kept at these two stations. Of the details of the work, Professors Rowland and Trowbridge will be able to state. It was in contemplation to make these the bases of an extended system of forty, fifty, one hundred, or a thousand stations throughout the country reporting to us systematically daily, giving us a daily weather map and electrical conditions of the earth and atmosphere throughout the country, parallel entirely with our daily weather map of isothermals, winds, and weather.

We took this up as a study from various points of view: (1), as terrestrial physics; (2), on account of its possible general connection with the weather; (3), partly for the special bearings these disturbance have on thunder-storms, &c.

While we were thus pursuing that course, which, in a few years, would perhaps lead to some results, the International Conference held a meeting in Paris this past April, and resolved that they desired to gather up the statistics daily, monthly, annually, as they best could—electrical statistics, as they may be called for by the country, and to make an annual report to the International Conference. That report being transmitted through the Secretary of State and the Secretary of War, comes down to the Chief Signal Officer as an order to carry out this work. So that our small beginning suddenly finds itself forced to expand to a larger undertaking.

We immediately, a few months ago, issued circulars to a number of the electricians of the country, and found that practical electricians, telegraph and telephone companies were in accord with our desire to investigate, observe, and map out the daily weather conditions. Some went so far as to say that they had been doing a great deal of work of the kind that had not been published. So far as letters and personal conference with them go, we have every assurance that they have at heart the same interest that we have. There has therefore been addressed to the commission, and through the commission to this convention, a general letter stating that the Chief Signal Officer desires to obtain as many reliable observers as possible for that phenomena bearing on atmospheric and terrestrial electricity by daily telegraphic reports, or weekly, monthly, or annual reports, as they can best send them in. He is willing to co-operate, as far as may be possible, in gathering these statistics and utilizing them for the public benefit, as well as for the special benefit of the electric companies, if they can make any use of them.

And as indicating a part of the scheme that he had in hand, which is nothing less than the covering of the whole country with electric stations, a memorandum has been submitted of eight or ten questions. Printed copies of these are already in your hands, and I will not detain you to read it.

We have, in conclusion, merely to request that these questions may be referred to proper committees. Other subjects, additional to those, and bearing on the same subject, may be reported on by these committees at some future meeting of the conference or commission, and I hope that we may have your hearty approval and co-operation, and your advice and actual observations as the basis for further research in the matter.

[Circular sent to electricians and various electrical companies in the United States.]

SIGNAL OFFICE, WAR DEPARTMENT,
Washington City, August 18, 1884.

SIR: I have the honor to say that I desire to obtain as many reliable observers as possible of atmospheric electricity, ground currents, telephonic electric disturbances, terrestial magnetism, and whatever other phenomena are related to terrestial electricity.

To this end I have requested of the Commissioners who have called the National Electrical Conference, to be held in September, that certain questions be considered and reported on by the proposed conference, but I take the liberty of thus addressing you in advance, in hopes by previous preparation to facilitate prompt action at the Conference.

The accompanying memorandum will show the scope of the questions which at present suggest themselves.

I am, very respectfully, your obedient servant,

W. B. HAZEN,
Brigadier and Brevet Major General, Chief Signal Officer, U. S. A.

I move, as a member of the Conference, that this communication from the Chief Signal Officer be referred to a proper committee, to be reported on within not less than three months.

Mr. TROWBRIDGE. I second the motion.

The ten questions called for and read are as follows:

The following subjects are suggested for discussion, with a view to recommending proper observations and reports:

1. What unpublished records exist in the hands of electric lighting, telegraph, and telephone companies relative to ground currents and atmospheric or auroral influences?

2. What is the general experience on east-west, north-south, and other lines?

3. What records can be kept by managers of all lines without interfering with daily business?

4. What special observations can be made?

5. What special lines can be, perhaps, wholly devoted to the continuous record of the phenomena.

6. Do or can the noises and currents, as observed on telephone and telegraph lines, give information as to the location and future movement of a thunder-storm, aurora, rain, cold wave, &c.?

7. Are observations on buried lines, or those covered with metallic tubing, or double aerial lines, specially desirable?

8. How can we best secure a complete daily electric survey of a given small portion of country, and a general survey of a larger region?

9. What is practicable in the way of securing a daily map of the distribution of atmospheric and terrestrial electric potentials?

10. Who will maintain self-recording electrometers?

Sir WILLIAM THOMSON. With respect to the kind of observation that is most important with respect to the so-called earth current, I venture to remark that an arrangement for measuring difference of potential between any points by means of high-resistance galvanometers taking really the functions of electrometers is the most important. The object is to measure the electro-motive force of the current, and not the strength of the current through the wire. The ordinary telegraph instruments show earth currents; that is, currents through the insulated wires and the instrument of the observer; and the practical suggestion that I have to make is that those stations which can undertake to do work of this kind should be provided with high-resistance galvanometers, galvanometers of twenty or thirty times the resistance of ordinary connecting lines, and even more. It may be a thousand or two thousand times. I believe generally the sensibility will be readily obtained by instruments having a resistance many hundred times greater than the resistance of the conducting wire.

Instruments of that kind, made self-recording, might be left in connection during all the time the telegraph line was not in use, and thus, during a considerable number of hours of the day, on even busy lines, an indication of the difference of potential between pairs of chosen points in the telegraph system might be obtained. It is the difference of potential and not currents through the wire that is the primary object of investigation at this part of the subject.

Professor F. E. NIPHER. I would like to have an additional point examined, and that is the effect which a large number of battery cells grounded at a given point would have upon the potential at that point. If these investigations were to be made at large telegraph stations that point would necessarily be examined first.

Professor ABBE. It was by no means intended in this short discussion to cover the whole ground, although many matters were put in that were thought interesting. I am sure that it is very desirable that all members should speak from their own experience, and suggest their opinions also, all of which should be referred to the same committee.

Motion carried.

The PRESIDENT. The next subject in order is the adoption of the international electrical standards. The discussion is to be opened by Professor John Trowbridge.

Professor TROWBRIDGE. The three delegates to the International Congress of Paris last April have received the following letter from the State Department:

SIR: I inclose herewith for your information a copy of a note from the chargé d'affaires of France at this capital, submitting for the adoption of this Government certain resolutions of the International Conference for the determination of electrical units, lately held at Paris.

I would thank you to consider this subject in connection with your colleagues, Professors Rowland and Barker, at your earliest convenience, and submit to this Department in the form of a report, the views of yourselves as the American delegates to the Conference upon the subject of the expediency of the adoption of the resolutions in question by this Government, adding that if you deem it necessary you may submit this subject to the consideration of the forthcoming Electrical Congress at Philadelphia.

Your obedient servant,

JOHN DAVIS,
Acting Secretary of State.

LEGATION OF FRANCE IN THE UNITED STATES,
Washington.

Mr. SECRETARY OF STATE:

As you are aware, the International Conference for the determination of electrical units, which suspended its labors in October, 1882, resumed them at Paris in the month of April last, under the presidency of Mr. Jules Ferry, president of the council, minister of foreign affairs, and concluded them on the third of the following month.

The president of the council has just sent me, for transmission to your Government, copies of the report of the proceedings of the said Conference, which I have the honor to transmit to you with this note.

As you will observe, the Conference unanimously adopted a series of resolutions, the object of which was to define with precision various electrical measures whose determination is of great importance to science and industry. At the same time, it expressed a desire that these definitions might be regarded as having the force of international law. Finally, it requested the French Government to transmit its desiderata to the various Governments.

These desiderata are contained in the document which also accompanies this note, and would constitute in a word the stipulations of the convention designed to secure international sanction to the propositions of the Conference.

The president of the council instructs me to invite your attention to this point very particularly, and he trusts that the United States Government will not object to sanctioning, diplomatically, these definitions, which have been unanimously adopted by eminent men of science, and which, without hampering the investigations of science, cannot fail greatly to contribute to securing greater safety and facility in those transactions daily becoming more numerous which have for their object electrical force and its various manifestations.

Here are the resolutions referred to:

(1) *Electrical units properly so called.*

Resolution 1. The legal ohm is the resistance of a column of mercury one square millimeter in section and 106 centimeters in length at the temperature of melting ice.

Resolution 2. The Conference expresses the desire that the French Government will transmit this resolution to the different states, and recommends its international adoption.

Resolution 3. The Conference recommends the construction of primary standards of mercury conformably with the resolution above adopted, and concurrently the use of secondary scales of resistance in solid alloys, which should be frequently compared with each other and with the primary standard.

Resolution 4. The ampere is a current whose absolute measure is 10^{-1} in electromagnetic units C. G. S.

Resolution 5. The volt is the electro-motive force that sustains the current of an ampère in a conductor whose resistance is the legal ohm.

(2) *Electric currents and lightning-rods.*

Resolution 1. It is desirable that the results of observations gathered by various offices should be sent each year to the International Telegraphic Bureau at Berne, which will prepare reports thereof for communication to the several Governments.

Resolution 2. The Conference expresses the desire that observations upon terrestrial currents may be made in all countries.

(3) *Standard of light.*

Resolution 3. The unit of each simple light is the quantity of light of the same kind emitted in a normal direction by a square centimeter of molten platinum at the temperature of solidification. The practical unit of white light is the quantity of light normally emitted by the same source.

Professor Trowbridge thought that the ohm, the volt, and the ampere having been adopted by all countries except the United States were now beyond discussion. The construction of primary standards should be referred to a committee charged with the establishment of a Bureau of physical measurements or electrical units. The point in regard to electrical currents and lightning-rods fell properly under the subject introduced by Professor Abbe. There remained, then, properly for discussion the standard of light. He presented these resolutions to the chairman, and moved that the adoption of the units be referred to a committee.

The President asked for remarks on the ohm, and said that 106 centimeters did not represent exactly the result of experiments, $106\frac{1}{4}$ being nearer; but the round number was thought desirable.

Sir WILLIAM THOMSON. The general opinion at Paris was that 106.2 is nearer the truth than 106, though able men who had made careful experiments went on the other side of 106; some as much as one-tenth and two-tenths, some even three-tenths below 106. The average, however, came within a very small fraction of 106—less than one-tenth. The thing that seemed to be quite sure was that 106 was nearer the truth than any other integral number, though most persons present seemed to think 106.2 to be the nearest decimal. The adoption of 106 was not considered to be practically inconvenient, even though more extended observations may give a number different by one, two, or even three tenths. A box of resistance coils, or a set of standards, put together in any way, of German silver or of any other metal that might be adopted for standard resistance, would still remain a useful practical instrument for measurement. It would only be necessary to alter the designation on the label of the box as to the temperature at which the resistances contained in it are correct. For practical purposes even, where measurements are not of the very most minute accuracy, any difference due to difference of temperature must be taken into account. Even if the material be German silver, there is a change of resistance amounting to three-tenths per cent. per ten degrees centigrade, in round numbers; and there is still more for copper — roughly, ten times as much as for German silver. Thus, by simply changing the designation as to the temperature at which the coils were accurate, which is within the limits of the atmospheric variation within which the resistance coils have to be used, the coils would remain a practical instrument, in the use of which no numerical correction would have to be applied, except such as must be applied at any rate—that is, the correction corresponding to the temperature at which the experiment is made.

Professor Rowland's experiments were then within two-tenths per cent. of 106 on the side below. It was regretted at Paris that he was not able to lay his last results before them. I hope Professor Rowland can now give this Conference those results, if not inconvenient or prejudicial to final figures.

The President called on Dr. A. L. Kimball to state the results of the latest experiments.

Professor KIMBALL. I would state that the first method employed in determining the unit of resistance was the same as that employed by Professor Rowland in his experiments in 1876—a modification of Kirchoff's method—by which the induction was produced by the reversal of the primary current. We have made a good many experiments with a set of coils constructed on purpose. These were 1 meter in diameter and were at a distance of 25 centimeters apart. There were seventy-five results obtained from these experiments. The first of these were not so complete as the later ones, so that in averaging we have given three or four early experiments the same weight as one of the later set, for

reasons which will readily appear in looking over the actual observations.

The result obtained thus far as most satisfactory, considered from internal evidence, is that one B. A. unit is 0.9864 of the absolute unit. That is a result which we feel confident is not in error more than one part in three thousand, and probably less.

Experiments have also been carried on for the determination of the value of the specific resistance of mercury. The result obtained is that a column of mercury one meter in length, and one square millimeter in section, has a resistance which may be expressed by the decimal 0.9539 in terms of B. A. units. Combining these two results, we find as the specific resistance of mercury in absolute units, a number which indicates that the absolute unit of resistance will be a column of one square millimeter in section and 106.278 centimeters in length. This result, as will be seen, agrees very closely with the results obtained by Lord Rayleigh and Mr. Glazebrook in their experiments, and we feel confident that it is correct to within about one part in 3,000.

There are some experiments which have been conducted by the method of Lorenz, the reductions of which are not yet quite complete, but have so far progressed that we can say that the extreme difference between the greatest and least values obtained by that method is less than one part in one thousand.

The PRESIDENT. It would seem from this that our experiments will finally give about that figure, 106.28. That is very nearly the mean of the best results obtained by foreign experimenters, Lord Rayleigh and Mr. Glazebook. Our old experiments, as Sir William Thomson has mentioned, came below this. We have found a cause of error in the sinking of the wire into the layer below when one winds it, and which I had allowed for, but not enough. Therefore I think when we correct the old experiments for that, as we shall presently do, the result will come out about this quantity.

I do not think a practical man will object to an error of that amount, viz, one part in 400, in the determination, say of the work of a dynamo machine. I would like very much to hear from some of the practical men here as to whether this quantity will affect their results to any appreciable extent. I think myself it is desirable to have a standard in conformity with the rest of the world. If we wish to be more exact, we can always make the small correction.

Sir WILLIAM THOMSON. The adoption of the legal ohm just now is, after all, only provisional. But the Conference at Paris was very anxious that the provisional character of the legal ohm should not in its results leave any uncertainty whatever that could practically inconvenience or interfere with accuracy in the measurements that are used for practical purposes. It was remarked that scientific observers always have a remedy in themselves. Scientific work is essentially a law to itself in respect to accuracy. But it is exceedingly important to the

scientific observer that the ordinary standards he meets with should be in sufficient approximation to correctness to allow him to use them without much labor in the way of equalization, and in the way of getting desired results and comparisons. I think that if the Paris Conference had had Professor Rowland's last results it would have adopted 106.2. The temporary provision of 106 will not be practically inconvenient, but it will be interesting to look forward to the discussion a few years hence, which will quite suffice to enable us to adopt a legal ohm as surely within $\frac{1}{20}$ per cent. of the truth as 106 is within $\frac{1}{4}$ per cent. of the truth.

Professor JOHN TROWBRIDGE. I should like to move that the Commission be authorized to take the proper steps towards legalizing the ohm in this country according to the recommendations of the Paris Conference.

Seconded and unanimously carried.

Professor TROWBRIDGE. I move that the unit of current, the ampere, which is one divided by ten in the C. G. S. system, shall also be referred to the Commission to take proper steps for legalizing this unit.

Seconded and carried.

Professor TROWBRIDGE. I would also move that the Commission be authorized to take the proper steps to secure the legalization of the unit of electro-motive force called the volt. The volt is the electro-motive force sustaining a current of one ampere in a conductor whose resistance is the legal ohm. I do not think that "legalization" is the proper term; "recognition" is perhaps better. I move that it be referred to the Commission to secure its final recognition by the Government. I presume all the Government can do in this matter is simply to say what these terms shall mean. That makes them lawful.

Seconded and carried.

Professor TROWBRIDGE. In regard to the resolution which reads, "It is desirable that the results of observation gathered by various offices should be sent each year to the International Telegraphic Bureau at Berne, which will prepare reports thereof for communication to the several Governments," it seems to me that it belongs to the Signal Service essentially, and that the committee recommended by Professor Abbe is the best committee to discuss and take proper steps for the adoption of this resolution. I therefore move that this resolution be referred to that committee.

Seconded and carried.

The PRESIDENT. The next question is the standard of light. I suppose this will occasion more discussion than anything we have had so far. There is considerable objection in the minds of many to such a standard as platinum which has just been melted and is about to solidify. I would like some motion upon this subject, so that the matter can come before us and be discussed.

Professor TROWBRIDGE. I should like to move that this resolution on the standard of light be referred to a committee. There is a great difference of opinion upon this unit of light, and very many do not see how it can become a practical standard, although a late modification by Mr. Werner Siemens, it seems to me, makes a very reasonable standard of it. Mr. Siemens takes the point of fusion of the platinum strip instead of the point of solidification. He passes an electrical current through his strip, and regulates the current until the very moment of fusion, when he measures the light through a small conical opening, with certain restrictions. But I do not know of any one besides Siemens who has actually tried the experiment. He obtains in that way, I believe, a light of about 1.5 candle-power, and he uses that as a practical standard. However, the question is evidently open for discussion. I have heard many express the opinion that this unit is not the very best that might be adopted, but it seems to be the only one of the resolutions adopted by the Paris Conference which we can practically discuss. Therefore, I move that it be referred to a committee who shall bring the discussion before us, and shall so frame their resolutions that the discussions shall not be desultory, but more or less systematic.

Mr. C. H. Koyl wanted an early date for the committee to report.

Professor TROWBRIDGE. I think many of these subjects had better be discussed by the Conference after the committee has framed certain resolutions so that they can be discussed. I think that the committee can report in three months, say at Washington, or some other place, after they have had all the experience which this present exhibition will afford them, as there are to be quite exhaustive experiments on the different dynamo machines. Of course, we cannot reach these experiments in a few days. All the committee can do is to open the discussion and draw up some rule confining the discussion so that it shall be valuable. I would add to my motion that this committee be nominated by the chairman and be announced to-morrow.

Mr. N. S. KEITH. As a member of the board of examiners, intrusted with the duty of measuring the light given by the various electric lamps in the exhibition, I am very desirous of hearing at this stage of the proceedings, or as speedily as possible, an expression of opinion, as wide as it may be, not perhaps so much for final action in this matter as for the guidance of this board of examiners. We have already had a little discussion upon this point, and our deliberations, our measurements, and our actual performances will certainly be regulated very much by such an expression of opinion. Therefore I do hope, without now being prepared to make an amendment to the motion, that the committee will so report that we shall have a short discussion, to say the least, upon this point before the Conference adjourns.

Mr. KOYL. We have not been informed what has been done upon the subject, for instance, what is the constancy of the light given out by a single centimeter of platinum, supposing it varied a tenth of a degree

from the point of solidification. Subjects of that kind are always important, and I hope will be brought up in this discussion.

Professor ABBE. I think that it is an admirable suggestion (and I would like to have it made the rule, to apply back to the committee suggested a few moments ago on atmospheric electricity and earth currents) that the committees be required to make a preliminary report at this present session, and receive such aid as may be given to them, and to make another report three months hence. I put that in the form of a motion—or an amendment to the motion made a few minutes ago—that the committee on light make a preliminary report to this association at this session, and that this be made to apply to all committees.

Seconded and carried.

Mr. W. H. PREECE. I want to propose that there shall be referred to one of the committees already designated, or to a new committee, the question of the consideration of the adoption of a new unit of power. The rate of doing work by dynamo machines and of currents generally has hitherto been very largely spoken of as being so many volt-amperes. But two years ago the late lamented Sir William Siemens proposed a unit of power called a watt, synonymous with the volt-ampere, and one that was so necessary for the purposes of practical electricians that in reality it has been adopted already. But it has not been adopted authoritatively. I was very anxious, indeed, at the Paris Conference to bring this unit before them, and I believe on that occasion I should have been supported by Sir William Thomson. But the foreign element was opposed to its introduction, first, because we already had so many names, and secondly, because there was no "w" in the French language. We have a very much stronger reason why it should be adopted—the reason of necessity. Therefore I propose that the subject of the adoption of the watt as the unit of power, or the unit of rate of work, shall be referred to one of these committees.

I want to go still further. The watt is a unit that connects together all practical applications wherever energy is expended in any shape or form; the rate at which it is expended can be expressed by this "watt." Hitherto, for the same purpose, we have used a most unmeaning and absurd unit called a horse-power, that is not a horse-power. Watt himself determined that the amount of work done by a good average Clydesdale horse was something like 22,000 pounds raised 1 foot high per minute. He thought that too small, and made it 33,000. Hence a horse-power, as at present, used means absolutely nothing, and if you have to convert the work done by an electric current, or the work done in an arc lamp or in a dynamo machine, to a horse-power, you have to use a very awkward coefficient—746; that is, there are 746 watts in a horse-power.

I have spoken with some of the most eminent mechanical engineers, and they all assure me that there would be very little practical difficulty in raising the value of the horse-power. If we were to make the pres-

cent horse-power equivalent to 1,000 watts it would mean that we should raise its present value about 34 per cent., and we should then have a true unit. You may still call it a horse-power. We should have a unit that would be connected directly with the absolute standard of measurement, the C. G. S. system, to which so many references are made.

Therefore I move that a committee be appointed to consider the subject: First, shall the watt be adopted as an authoritative unit of power? and, second, shall the value of the horse-power be increased to 1,000 watts?

THIRD DAY.

The Conference was called to order by the president at 3 p. m., September 10.

The PRESIDENT. The announcement of committees is now in order. The committee on the subject of electric light measurement, and upon the consideration of Mr. Preece's motion with respect to the watt, is as follows:

Professor John Trowbridge, chairman; Mr. W. H. Preece, Mr. E. C. Pickering, Professor C. R. Cross, Professor G. F. Barker, Mr. T. A. Edison, Major D. P. Heap, United States Army.

The committee to consider Professor Abbe's subject is as follows: Professor Abbe, chairman; Lieutenant James Allen, Professor J. Trowbridge, Dr. W. W. Jacques, Mr. F. N. Gisborne, Mr. W. H. Preece, Professor F. E. Nipher.

It is purposed to add one or two more to this committee to represent the telegraph companies of this country. Being unfamiliar with the proper persons, these names have been left to be added. These committees will make a preliminary report, to-morrow morning, so that these subjects can be brought up for discussion by the Conference. I am myself more in favor of having these questions discussed here than I am of having them left entirely to these committees.

The next business in order is the first subject on the programme, "The establishment of a National Bureau of Physical Standards," the discussion to be opened by Professor Monroe B. Snyder.

ADDRESS OF PROFESSOR SNYDER.

MR. PRESIDENT AND GENTLEMEN:

The establishment of a Bureau of Standards adequate to the scientific and practical needs of the nation is a topic that may well claim the attention of the public at any time. There are, however, several reasons for bringing the question before this National Conference of Electricians.

The International Commission having recently determined what the international electrical standards are to be, it remains for this country

to give proper and official attention to the important matter of recognizing and establishing such standards.

And again, it is found, at the very outset of a movement in this direction, that the definition and construction of the electrical standards necessarily involves the definition and construction of a number of other physical standards; since to attempt the establishment of a standard of resistance, for example, without reference to standards of temperature, length, or even of mass, would be to fail in the first statement.

But more than all, with the presence at this Conference; of physicists, of practical experts, of manufacturers and others interested, we may expect that this matter will receive the many-sided consideration which its importance deserves.

It may be well at the outset to glance briefly at the present condition of the subject of physical standards in this country. Standards of length and of mass have long since received considerable attention from a department of the United States Coast Survey. This Bureau is, I believe, sometimes designated as the Bureau of Weights and Measures, and furnishes standards of this class to the several States.

The original intention of attaching such a Bureau to the Coast Survey was undoubtedly to provide proper standards for its important geodetic work, and in pursuance of this intention the Bureau has undoubtedly been useful.

We find, however, at a later period the department of the Lake Survey independently taking up the subject of the standards of length in a very spirited manner and with a success creditable to those engaged in the work. It is also understood that the Coast Survey itself has recently been active in some improvements in this department.

As for the other standards connected with Government operations we have notably the attempts of the Signal Office to determine for their work standards of pressure and of temperature, and we are also informed that in some of the Government work preliminary electrical standards have been adopted, though doubt has been expressed as to whether the same standard is used throughout the entire service, and, as a matter of course, is still more doubtful whether these standards harmonize with the present state of electrical science.

It will be noted that all these attempts on the part of Government officials to establish physical standards, creditable as they may be in themselves, have been mainly with the view of meeting the necessities of their own particular departments, but are scarcely available for the scientist and the practical public.

Outside of the Government Departments private enterprise has, indeed, done much to further the study of this subject, and in several educational centers most laudable efforts have been made to establish and reproduce standards of length and of temperature. Thousands of dollars have been expended by individuals and institutions in this direction,

and yet with all deference to, and recognition of, this self-denial and enterprise, it can scarcely be supposed that the private purse will be equal to the support of the amount and the character of work that yet remains to be done in this direction. While, therefore, too much cannot be said in praise of these individual attempts, whether undertaken in the Government service or by private effort, to lay a firm foundation for our science and our practice, there still remains the necessity for the establishment of a National Bureau of Physical Standards that shall at once unify all the standards of the various Government Departments, and also be equal to the real wants of progressive science and actual practice in this country.

It is perhaps important that we indicate in brief and in outline the character of the Bureau intended.

It should, in the first place, aim to determine and reproduce all the physical standards with relation to each other. In the present state of science it is so manifestly absurd to determine or even to state one standard without respect to the others, that it does not seem necessary to emphasize this point. Still we may, in passing remind ourselves of the fact that, as the matter now stands, one must often be in serious doubt as to precisely what subsidiary standards were used in work where observations are not made at a physical zero. Such Bureau would then provide for standards of length, of mass, time, temperature, of electrical resistance and electro-motive force, of light, and also probably provide for other subsidiary standards necessary to the great business interests of the country. In the second place, the proposed Bureau should not only be broad in scope, but should thoroughly provide for two distinct and yet related features.

First, the standards should be the embodiment of all the scientific research at present available in these matters. Some plan should, therefore, if possible, be devised to eliminate the personal equation which, even in matters of high scientific research, becomes at times unfortunately too prominent. There seems, indeed, no good reason why such a Bureau should not avail itself of all the experience and research in these matters that can be supplied by the trained physicists of this country.

Then, again, methods and means should be provided for furnishing the public with authoritative multiples and submultiples of such standards.

This means not only that research should be undertaken for determining the best methods of copying, multiplying, and subdividing standards, but that means should be provided by which the public can readily obtain such copies, multiples, and mulsubtiples. It should not be difficult for any one, whether engaged in scientific or practical pursuits, to obtain standards of an accuracy equal to the purpose for which they are desired.

It seems, moreover, necessary to the effectiveness of such a Bureau

that it should have the stamp of Government sanction and of Government support.

Excellent from a scientific standpoint as some of the standards championed by individuals and institutions may be, there is yet wanting the stamp of authority to make these researches definitely valuable to the community.

On the other hand, the standards determined by Government departments have indeed the advantage of quasi-governmental authority, but are, as previously stated, in such a shape as not to be either strictly related to each other or practically accessible to the public.

The character of the Bureau here intended should be also defined as incompatible with any present department of the Government service. It cannot be supposed that a Bureau of Physical Standards of the character and scope here intended could be either scientifically or practically supported by any one of the present departments of the Government service. It should, therefore, in our opinion, both on account of the practical and scientific interests at stake, be both in its constitution and in its business and scientific management a department of its own.

In order to complete a Bureau of the character described it would also be necessary to have associated with it a physical laboratory and observatory of no mean pretensions, a laboratory devoted exclusively to researches connected with such work, an observatory that would carefully note the physical conditions under which the standards are preserved, and also, if possible, serve as a central station at which physical observations, to be made by the various departments of the Government service and by private individuals, may be compared and to which they may be reduced.

In short, we may say that this Bureau of Standards should occupy, with respect to the practical physics of the country, precisely the same relation that the Nautical Almanac Office holds with respect to practical astronomy.

And just as formerly Government aid was invoked and is now freely furnished for the observation and computation of data which form the basis of the actual work in the astronomical observatory and of the plain sailing of the mariner, just as Government aid has enabled the accumulation of data of every-day importance to the astronomer, the geographer, and the sailor, just so should Government aid now be invoked to furnish a Bureau for the determination, the subdivision, the preservation, and the development of a series of physical standards which shall be adequate to the urgent needs of the physicist, and also be available for the practical expert and the manufacturer.

Professor WILLIAM A. ROGERS. Mr. President and gentlemen of the Conference: It needs to be remembered in the discussion of the question under consideration that while a good advance has been made during the last ten years in the definition and value determination of physical units, there is much to be done in the way of auxiliary research before

the physicist can have within easy command the means of reproducing these units in the most convenient and practical form. If the question relates simply to the establishment of a Bureau of Weights and Measures, in which legal units shall be carefully preserved and exactly compared with other corresponding units, there would seem to be no need of any other organization than that which is now under the direction of the Coast Survey; but in view of the present demands of electrical science it almost goes without saying, first, either that the scope of the present organization of the Bureau of Weights and Measures should be greatly enlarged; or, secondly, that a new department under Government sanction and control, but under the highest scientific direction, should be established, to which every inquiry relating to units may be referred; or, thirdly, that the aid of private munificence should be sought for the establishment of a Bureau of Physical Units under an organization which would command the respect and confidence of the physicist, not only of this country but of the world.

The general current of opinion seems to be in the direction of an organization as a separate department of the Government rather than as an attachment to either of the present scientific organizations, however admirable the work which has been done in the fields of research which they have occupied. The standards department at London is not at the present moment particularly well equipped with the appliances of investigation, but it may well be taken as an admirable illustration of the way in which scientific research may acquire the stability and authority which Government sanction can add to the results of patient and careful research, without interfering with perfect freedom in scientific inquiry and in special scientific investigation. At Breteuil, notwithstanding the complexity of the organization of the International Bureau, there is perhaps greater freedom of action in this respect.

Whether rightly or wrongly, Government sanction does add something to the most conclusive argument from a scientific standpoint. Suppose I charge myself with the investigation of the exact length of a certain yard or meter of a given temperature. As the result of a labor extending over several months, I make a report over my own signature. Whatever real value this report might have, it rests upon a purely personal basis fortified by such evidence as the report itself might furnish; but the same work, done exactly in the same way, under exactly the same conditions, and with the same instruments, if performed under a Government appointment, acquires an authority which would command for it a degree of confidence which could be obtained in no other way. This fact may not be particularly creditible either to Government or people, but it represents a state of public opinion of which we must take cognizance at least at the present time.

But while we admit that a Bureau of the kind proposed should be in some way under Government sanction it is necessary that the limitations of that sanction shall be distinctly recognized and clearly defined. Under

the sanction of law the Bureau could properly define all natural physical constants and the entire units of length and weight, whether natural or arbitrary. In other words, the Bureau could be the guardian of original units, but it could not properly define those units under another form or under other conditions or as either subdivisions or multiples of the original units except in just so far as special investigations in a given case are in exact correspondence with the natural laws which affect those units. This limitation of Government authority is well illustrated by the rather humorous edict of the Archbishop of Lausanne, in 1479, in which he excommunicated, "ye obnoxious worms and grubs that nothing shall be left of ye *except such parts as can be useful to man.*" In this case the Government ordered its officers to report the consequences of the excommunication, but the saucy chronicler says "that no success had been obtained, probably on account of the sins of the people."

The Bureau proposed could properly define the length of a meter upon any metal of a given temperature, and of a yard upon the same or a different metal or at any other temperature, but it could not define the relation between the yard and the meter at a common temperature, except by making a determination of this relation in a given case. The Bureau could not define a foot or an inch, a decimeter or a centimeter, except by making an exact determination of their subdivisions of the entire units in a given case. There, then, can be no such unit as a Government foot, a Government inch, a Government decimeter, a Government centimeter, a Government ounce, or a Government degree of temperature.

Is it not clear that all of these subunits belong to the domain of investigation? And is it not equally clear that physicists and electricians of this country need a well-equipped and well-appointed Bureau to which all such questions may be offered? And should not that Bureau, while under the direction and responsible control of the Government, be under such scientific direction as would give perfect freedom of action in all matters which relate to purely scientific investigations, so that when a given résult is announced it will command the confidence of every physicist, whatever his special field of research?

If this could be accomplished we should save the present waste of labor in duplicate investigations, and I suspect we should have also far less search after the unattainable.

It may be worth while to point out some of the directions in which investigations conducted by a Bureau of the character proposed would be extremely useful, and one might almost add, imperatively necessary.

In the matter of thermometric measurements investigations are still needed to determine the cause of the effect of age in producing a rise in the zero point. Does the length of the glass tube itself change with age? I doubt it. Have we at the present time any precise method by which the errors of thermometers, at very low or very high temperature, can be determined? I notice that in certain tests to be made at this

exhibition it is required that a temperature as high as 3,000° Fahrenheit shall be determined. I have myself tried in vain to devise some method by which the real temperature of the interior of a mass of metal may be ascertained. I leave to others who are more familiar with the demands of electric science to point out the directions in which investigations of this kind are needed, but in the matter of units of length there is much work which needs to be done before the physicist can obtain in a convenient form the short units of length which he needs in his daily work.

First of all, since the subdivisions of a legal unit of length can be made with the greatest precision when they form an integral part of the entire unit—that is, when they are laid off upon the same metal as the entire unit—we ought to have in this country a platinum-irridium meter of the same form and dimensions as the prototype of the International Bureau at Breteuil, but this meter should be subdivided at least into decimeters and centimeters. On account of the great cost of the material it would not be practicable to use this metal in the multiplication of copies of this standard.

Hence we should need to determine the best material for such copies, the best shape, the best dimensions, the best mass, the best form, of service for receiving the graduations.

Suppose the physicist should require the absolute distance between two points expressed in terms of a meter upon platinum-irridium, which bears a known relation to the prototype of Breteuil, the distance being less than an entire meter.

Unless the well-nigh impossible condition be secured of absolute uniformity of temperature for a period of time varying between twenty minutes to six hours, according to the composition, the shape, and the mass of the material employed, the exact determination becomes a matter of extreme difficulty.

The subdivisions of the whole unit employed cannot be determined with accuracy unless they are laid off upon a plain surface, the metal bar being supported at two fixed points. Again, the time required for heat to produce its normal effect upon bars of different material varies with the material, the shape, and the mass. The rate of propagation of heat-waves, and the time which must elapse after a change of temperature takes place before a comparison can be safely made, must be determined for every bar of metal which is to be used as a secondary standard.

The physicist often needs an extension of the legal unit of length, especially in geodetic work. Will any one undertake at the present time to determine a steel tape 100 feet or even 10 feet in length? Such a determination would require the production of a plain surface not less than 20 feet in length. Mechanical methods of producing such a surface are within reach, but we need some method of testing long plane

surfaces at all comparable with the optical methods applicable to plane surfaces of small dimensions.

I need not multiply the indications of the directions in which many other kinds of serious work needs to be done. They are to be met with in the daily experience of the physicist. A thousand new demands are made with every extension of the field of physical research. I shall be quite content if in these few words I have succeeded in impressing upon the members of this conference the gravity of the demand which the physicists of the country make upon the Government for the establishment of a Bureau in which all the questions which relate to standard units may be patiently and intelligently investigated, and, as far as possible, completely solved.

Professor J. E. HILGARD. There can be no doubt that the Government must have standards, but the extent to which the Government should assist in the production of standards or in furnishing them to the public is a matter for discussion. My own view is that nothing of this kind can be defined by legislation. The Government can only enact what shall be held in the courts for the proper maintenance of contracts. I should deprecate the establishment of a Bureau at which these things were done by the Government. In my judgment that kills enterprise and investigation. I want from the physicists of the country, in the university, and in every laboratory the process of investigation to be going on. Nobody can fix a physical constant, in my judgment, for one hundred years to come.

If the Government, however, has the best adapted apparatus attainable, and undertakes merely to compare and verify such measures as are manufactured by other people, and which come to them for a standard, then the Government does that which the British Government does, and what we do in regard to measures of length now.

I should very much prefer to keep up that spirit of emulation, competition, and investigation which now exists in the country than to do anything which would paralyze it by setting up some particular person in this matter who would have the authority to say "that is right."

You can do that thing in law, but you cannot do it in physics. I will recall with a little emphasis a case which Mr. Rogers has very appropriately quoted but not explained as well as might be, namely, that the British Government actually enacted by act of Parliament under Victoria Regina two different things at one and the same time: 1st, that the yard should be such a part of the meter, and that it should be such a part of the length of the seconds' pendulum at Greenwich. It proved neither to be that part of the meter nor that part of the seconds' pendulum at Greenwich. In other words you cannot enact physical standards; but you can say what may be held in the courts in case of a litigation. It was quite sufficient when this Government followed the British Government in making the use of the metrical standards optional to say that contracts may be held by the courts in such a case and the me-

ter shall be considered 39.37 inches; that is quite enough for practical purposes; it was not worth while to go into the thousandths of a meter.

I take it that no physical standard is yet fit to be, or even very nearly fit to be, enacted into a law. With regard to the discussion which we had yesterday about the 106.278, I think we should ask the Government, in that legal definition which should be used in cases of controversy, to accept 106 short of the decimals as the only percentage that is needful in all business transactions; that is, if the Government has anything to do with it. It has nothing to do with the developments of science, and an establishment of this sort I do not think will find favor at the present time. To go a little further, let us have what was proposed yesterday—national electrical standards, defined by law in such a way that they may become magnitudes that can enter into contracts in which you can sue and be sued. The business of Government is to maintain equity among men, and not to define the minute qualities of matter and the relations of electrical units.

I have different views from those that have been expressed here before me in regard to what should be the functions of a National Bureau of Standards, but I am still of the opinion that a National Bureau of Standards should be charged with the obtaining and preserving of electrical and other units; and I confess, since they do not now remain constant, that they should be compared from time to time. They may improve the methods of determining them, but let us have no enactment about it. It is wrong to say that Governments do not concern themselves about this thing. We were participants in the International Bureau of Standards of Weights and Measures at Breteuil, which is intended to be, and is at the present time, an establishment that will meet every requirement of physical conditions under which determinations can be made. It is a very costly thing, and it was thought by the twenty States that established it that it was much better to have one at a common cost, that was as perfect as possible, than to have twenty which would necessarily be less perfect.

I have received, since I commenced my attendance here, information which I state publicly for the first time, that the announcement has been made by Lord Granville to the president of this Bureau that England, which had stood out hitherto, has come into the convention for the establishment of the International Bureau of Weights and Measures. We are, therefore, well taken care of when it comes to ultimate accuracy; but it must be observed, I am free to say (because it is well known that I often have occasion to use in public business the greatest precision possible), that there may be such a thing as overdoing that matter. We measure base lines of ten miles say, for triangulation, with such precision that an error does not exist of a millionth part of the whole length. That has been several times accomplished, not only in this country, but in Spain and Switzerland. It does not require much knowledge of triangulaton or of surveying to know that at the very first apex

which we intersect from this base line, we lose all that superabundant accuracy. We cannot absolutely intersect a point with angles and lines, and to know ten miles within a quarter of an inch answers all the practical purposes of surveying. We can lay down one object for such accurate measurement of a line (and I presume the parallel will be found precisely in this electrical business) namely to measure at intervals, and with precision, earth lines of considerable length, a kilometer or ten kilometers, in order to establish the connection between materials which we use in accurate measurements and the globe itself. That, perhaps, is more constant. With this view we have established near Washington, at Fort Myer, as it is now called, a standard kilometer which we had measured with the geatest precision. This was begun four years ago, and I am free to say that we have not yet satisfied ourselves about the permanency of the marks, although they are ten feet under ground, and are only seen from above through a tube which is ordinarily closed. It appears to us that we shall have to use more precaution in the way of securing the permanency of the ground. We did all the thinking about it in the first instance that we could, but experience shows that we did not protect the marks sufficiently. Water accumulates around them; and, although there is no evidence of displacement, they cannot be assumed as constant.

This is one of the means which a Bureau has of maintaining and checking its standards from time to time. I suppose that if we had the standard yard of three thousand years ago, or what was spoken of at the time as the pyramidal cubit, a great deal of interesting information might be obtained which is lost to us.

The PRESIDENT. I suppose the immediate object of bringing this subject before the Conference is to obtain some place where electrical standards can be compared. So the discussion had better refer as much as possible to that, although perhaps the whole thing hangs together. I will now call upon Lieutenant Allen of the Signal Service for his views upon the subject.

Lieutenant ALLEN. Realizing the necessity for the existence of a Bureau of the nature described by Professor Snyder, or as modified by Professor Hilgard, it is not unreasonable to expect that the efforts of this Conference to secure its establishment will meet with speedy success. As, however, even with the most favorable progress a considerable time must elapse before the Bureau will be able to put into the hands of the investigators practical, working models or copies of established standards, it may not be uninteresting to you to know what has already been done towards securing standards in two important subjects, as one of them—temperature—is so closely related to electrical standards.

I will, therefore, present to you a brief statement of the work done by the Signal Service towards securing standards of thermometry and barometry. Being unable to obtain satisfactory ones in any other way,

the Chief Signal Officer began an independent determination of standards for the Signal Service, and also had made careful and extensive comparisons with the standards in Europe.

The air or perfect gas thermometer has been adopted by the Signal Service as the standard for temperatures. Comparisons have been made with an air-thermometer and two mercurial thermometers from —38 F. to 0 F., and with six mercurial thermometers from 32 F. to 122 F. The air thermometer has also been compared with three alcohol thermometers from —71 F. to —38 F.

These various alcohol and mercurial thermometers are compared with the working station-thermometers of the Signal Service before they are sent to their destinations.

Until two years ago none of the Signal Service thermometers had ever been compared with standards at low temperatures. A few of the thermometers had corrections assigned, sometimes as low as 12 F. The comparisons that are now being made at low temperature show that the minimum alcohol thermometers that have been in use read too low at —40 F. by about 4 F., on the average. Sometimes they are found to read too low by as much as 6 F. and 8 F. The one that has been found farthest out so far reads 12 F. too low at a temperature of —56 F. The mercurial thermometers at very low temperatures —35 F., sometimes read higher and sometimes lower than the true temperature by about 1 F. to 1.5 F. Occasionally, however, one is found that reads out as much as 3 F.

These large discrepancies are not surprising when it is considered that the makers have no reference to marks below 32 F. The old thermometers were pointed by the makers at 32, 52, 72, and 92 F., and the remainder of the graduation above and below made uniform without any reference to the varying diameter of the bore of the tubing.

The thermometers now being made for the Signal Service show that when carefully used its indications can be relied upon within 0.1 F. The temperature at which the air thermometer has been found to differ the most from the mercurial thermometer is from 90 F. to 100 F. The mercurial thermometer at those temperatures reads higher than the air thermometer by about 0.3 F. Some thermometers of a new kind of glass made by Green, of New York, differ not more than 0.1 F. from the air thermometer from —38 F. to 122 F.

Below 32 F. the difference between the air thermometer and mercurial thermometer increases as the temperature diminishes, and at —38 F. two Baudin thermometers were found to read lower than the air thermometer by 0.4 F.

It is satisfactory to be assured that the mercurial thermometer is trustworthy down to temperatures very near the verge of the freezing point of mercury. In the comparisons of the mercurial with the air thermometers the observations were made at a few temperatures, principally from 72 F. to 112 F., with some results from 32 F. to 72 F., and at low

temperatures from —38 F. to 0 F. On any one day comparisons can be made at only a few points on account of the amount of labor involved in determining the freezing point of the air thermometer. The results to be good must be combined with a freezing point observed the same day comparisons are made.

The finally adopted corrections of the thermometers to reduce to the air thermometer have been obtained from an adjustment by least squares of the observed differences at the various points. The differences are considered to be due to the varying expansion of glass and mercury at different temperatures. A change of one degree at the boiling point causes greater changes of volume than the same amount of change at freezing point. If the expansion is supposed to depend on the second power of the temperature, theory shows that the maximum difference of air and mercurial thermometer would have to be at 122 F., or at least within a few tenths of it.

The maximum difference, however, is found about 92 F., so that something else must be the cause of this difference, or else the expansion of the glass and mercury depend sensibly on the third power of the temperature. A formula for the difference of air-thermometer and mercurial thermometer, based on this latter theory, was found to satisfy the observations very well; and the coefficients of expansion of glass depending on the second and third power of the temperature as determined from the observations, was found to agree very well with what is known of the coefficient of the second power of the temperature as determined by other methods.

The theory referred to, that the expansion depends sensibly on the third power of the temperature, shows with the constants derived for some of the thermometers that there is another point of maximum difference between the air and mercurial thermometer from 32 and 212 F. besides that at 92 F. mentioned before. On one of the thermometers this was found to be at 190 F., where by theory it would read a little lower than the air-thermometer.

No comparisons were made between the air-thermometer and mercurials as high as 190 F., as the requirements of meteorology do not extend that far. It is interesting, however, to note that in some comparisons made by Dr. Grunmach given in Metronomische Beiträge No. 3 a mercurial thermometer was found that read about 0.06 F. lower than the air-thermometer at about 180 F.

Using this theory, then, with constants determined from the observations for each thermometer, the difference between the air and mercurial thermometer was computed at intermediate points where no observations were made.

At very low temperatures—38 to 0 F., the difference between the air and mercurial thermometer seemed to show that the expansion of the glass or mercury does not depend sensibly on the power of the temperature, but is more nearly uniform.

The corrections of the thermometers as mercurial standards are based on very careful calibrations by Thieseb's method for twenty equidistant points on each of four Tonelot thermometers, from freezing point to boiling point, and determinations of their scale lengths from the depressed freezing points and boiling points.

This method of determining the scale length of a mercurial thermometer, as shown by Dr. Pernet, of the International Bureau of Weights and Measures, is to be preferred to the use of the freezing point of a thermometer observed before boiling point, because the scale length between boiling point and the depressed freezing point does not vary with the age of the thermometer.

The freezing points of mercurial thermometers are always rising with age. Exposure to high temperature lowers their freezing points temporarily. Exposure to 122 F. lowers sensibly the freezing points of the thermometers used as standards in the Signal Service. Some made recently by Green of his new glass, that show such a small difference from the air thermometer, show also very little change of freezing point when exposed to 122 F.

In comparisons of mercurial thermometers with the air thermometer and of mercurial thermometers among themselves, the freezing point correction to be used at any time in deriving the total correction is that observed immediately after exposure to the temperature for which the correction of the thermometer is required.

In a series of comparisons extending over a considerable range of temperature in order to keep the freezing points of the thermometers nearly the same at the different temperatures, the thermometers before comparisons were begun were first heated up to a temperature a few degrees above that at which the highest comparisons were to be made, for the purpose of depressing the freezing point. The freezing point was also observed after the series of comparisons. The two never differed more than 0.02 C. In reducing the observations, the freezing-point used at any temperature was that observed after exposure to the high temperature; or in case of a difference between that and the freezing point at the end of the series, an interpolated value of the correction was used for each temperature according to the time. This is in accordance with Dr. Pernet's custom also.

In comparing at low temperature also, the freezing point used was that observed after exposure to the lowest temperatures. Exposure to very low temperatures, even freezing the mercury in thermometers, did not seem to raise the freezing point materially. The greatest change for two Baudin thermometers was a rise of about 0.05 F. Possibly, however, if the exposure was long continued the rise would be much greater. The details of the work of comparison will shortly be published by the Signal Office as a professional paper.

Two compared mercurial thermometers, one made by Tonelot and the other by Green, may be found in the standards room of the exhibition.

BAROMETERS.

The ultimate standard to which pressures observed in the Signal Service are referred is the standard of Kew, England. A number of barometers made by Adie have been brought to Washington, and the mean of these after correction with Kew certificates is considered to accurately represent the Kew standard, and is used in comparing with the barometers sent to stations.

In 1882 and 1883 comparisons were made between a number of Fuess barometers belonging to the Signal Service and the standard barometers at St. Petersburg, Vienna, Berlin, Kew, and the International Bureau of Weights and Measures at Sevres. The mean of these Fuess barometers will thus transfer to Washington all the important barometric standards of Europe.

The results of all these comparisons are not yet completely known. The Fuess barometers were brought safely to this country and comparisons made between them and the Adie barometers in the Signal Office. The results of these latter comparisons show that the St. Petersburg standard differs sensibly from the Kew standard. The only instrument in the Signal Office approaching an independent standard is one made by Mr. Green, of New York. The tube has an interior diameter of about one inch. This barometer, however, is not very satisfactory. It is read by a vernier like an ordinary small barometer. The setting of the vernier disturbs the surface of the mercury so much that a very accurate reading cannot be made. The comparisons of this barometer, however, unsatisfactory as they are, after allowing for all its sources of error as far as they can be determined, show that as a standard it agrees much better with the Kew than the St. Petersburg standard.

Steps have been taken to procure a normal barometer of the most perfect type for the Signal Office, and to make a thorough study of the matter of standard pressure to be used as the final reference of all the Signal Service observations of barometers.

The work already done has greatly increased the efficiency of the Signal Service, but other and more accurate standards, especially in electricity, are required for the proper advancement of the science of meteorology.

The Chief Signal Officer directs me to express to the Conference his belief that a Bureau of Weights and Measures, to include all physical standards and to be recognized as legal authority in all Government work, should be established in Washington, and that it should so far as possible be in harmony with the International Bureau, maintained by the co-operation of all Governments at Paris.

Professor SIMON NEWCOMB. I have listened with great interest to Professor Hilgard, and I do not clearly perceive that there is any necessary antagonism between the views which he expresses and the views which have been presented by the two eminent gentlemen who preceded

him in favor of the establishment of a Bureau of Physical Standards. I do not sympathize with the feeling against the repression of enthusiasm in research of a certain kind which was expressed by Professor Hilgard. I think that if twelve men are engaged in doing for the country at large a work which one of them could do just as well as all twelve, provided he was officially responsible, it is rather advantageous to discourage the other eleven and to put the whole work in the hands of the one man.

Neither can I see that there is anything unreasonable in the fact expressed by Professor Rogers that greater weight may be given to the work conducted under official authority, than if it were done by the same person, as a private individual. When one is doing work which he knows is going to be received as definite and final by his countrymen, he takes a great deal more pains in consequence of that feeling of responsibility. There is a prevailing disposition to give too little weight to the sense of responsibility among men, and too look upon it as a secondary matter. I think that the question as to who is responsible should be considered as a fundamental question in every matter affecting the scientific interests of the public.

I think from the manner in which this discussion has been commenced that there is a feeling on the part of many members of this Conference that we are very much in need of some common set of standards to be used for electrical purposes, and some place to which we can send an instrument and have it tested and its variations determined. This is the function of the present Bureau of Weights and Measures, over which Professor Hilgard presides, and I don't see why the work of that Bureau should not be extended to include all physical standards. It might be made a branch of the International Establishment at Paris, but at the same time our electricians cannot send over to Paris in order to have their instruments tested. We must have some sort of a place in this country for that purpose, and our Government should establish under some one person's direction such a bureau as is here desired.

In this connection I would suggest that Congress at the closing hours of its last session passed a law looking toward the general reorganization of the several scientific departments of the Government. I think that this Conference should seize the opportunity of pressing this matter before Congress and suggesting such measures as the Conference in its wisdom may deem proper in order to bring this matter to a consummation in the United States.

Sir WILLIAM THOMSON. I have listened with a great deal of interest to Lieutenant Allen's paper regarding the thermometer. Many years ago the subject of thermometry was taken up in France by Regnault, and in England by the Kew Committee of the British Association, operating by aid of agrant from the British Association, and helped by the Government, not largely, but in one important respect by the bestowal on the committee of an observatory in the Royal Park of Kew.

The British thermometric establishment did most excellent work, and helped to raise the standard of accurate thermometry in a manner which has been exceedingly valuable for practical purposes. When the Kew committee commenced its work, it was no uncommon thing to find thermometers put forth by the best instrument-makers of beautiful workmanship and large cost, but which could not be trusted within a degree or a degree and a half Fahrenheit. That state of things was simply abolished by the action of the Kew committee. They used Regnault's and Joule's methods. They made thermometers in the first place, and in the second place gave out thermometers as tested by them which, although subject to the inevitable variation of the glass, and varying in their absolute value in a manner not to be predicted, yet were reliable as to difference of temperature through a wide range to within one-tenth of a degree. There can be no doubt that the maintenance of the Kew committee and its maintenance now, which I am glad to hear of, is of vital importance for thermometry. I do not know that thermometers are included in this motion, but in the United States I should think the comparison of thermometers would be a valuable part of the business of the proposed establishment.

We want the same thing for electrical instruments. At present we have ampere meters and volt meters, which are not good even within 2 or 3 per cent., and sometimes not within 10 per cent., of what they profess to be. There is great difficulty in obtaining instruments which have the conditions of constancy in themselves; and when any one has produced an instrument which has in itself the conditions of constancy it is exceedingly difficult to get the value of its coefficient determined. In fact, we have no way of doing it now. That may be seen by the numerous proposals that were made before the Paris Conference of various methods for finding, in absolute measure, the strength of any given instrument—the electrolytic method, the method by the rotation of the plane of polarization, and several other methods. The subject is in the condition in which any official action must be directed by science. And although the results of science are not yet fully obtained, yet scientific men in America, England, France, and Germany are just now working strenuously for them, and there can be no doubt that before any resolution of Congress can go into effect there will be from several sources in the several countries I have named means of carrying out in practice the requisitions that this Conference proposes to make upon a Government Bureau.

Our requisitions in respect to accuracy are of a very modest kind at present. We should be glad to have instruments which show potentials within one per cent. for ordinary use, and which may be trusted within one-half per cent. in more careful measurements. Who would not be thankful to have ampere meters which we could be assured would show anything from one ampere to a thousand amperes within one per cent.? This, I believe, will be available before another year passes. At

the present moment we have not the means of testing within one per cent. the value of the indication of any such instrument except by a process which is one of the difficult processes of the physical laboratory. I do feel, with Professor Newcomb, that the expenditure of labor is to be grudged if it involves working week after week and month after month by persons competent to investigate new properties of matter, if they are all doing the same thing. The repetition of that performance at Harvard College laboratory, Owen's College laboratory, Helmholtz's laboratory, Lord Rayleigh's laboratory, &c., cannot contribute to our knowledge, and cannot even promote accuracy. Lord Rayleigh has taken this matter especially in hand; and if I had an instrument that had in it the elements of constancy, and I could get Lord Rayleigh with his assistants and co-workers to undertake to give me its physical constant—to give me the numbers of amperes, within one-tenth per cent., which correspond to the indications of that instrument—I should feel that I would get something which would take two or three years of work to find for myself, and, further, something which I would feel confident I could not do in my own laboratory in the two or three years as accurately as Lord Rayleigh.

That seems illustrative of what we are to do in this matter—to elect persons competent to perform the fundamental measurements, and then make it easy for the rest of the world to make their comparisons.

What would we think if there were in each laboratory such comparisons of standards of length as Professor Hilgard has brought before us, or such comparisons as have been made by Miller at Cambridge of standards of weight in which he determines his weighings within one five-millionth by devoting years of his life to it?

The resolution before us is of the greatest importance in methodizing and putting a good foundation to the work that is to be done, in order to obtain the most accurate possible fundamental standards, and to make it as convenient and cheap as possible for every practical electrician to compare his instruments with the standards. You have here a free country, that has the good of science at heart, and wishes to do good in its own borders and over all the world. I venture to suggest that the work in the United States should not be confined to one Central Bureau, but that it ought to look forward to having as soon as possible inspectors of electrical measures, such as ampere meters, volt meters, and so on, in the various cities in the United States and all the important towns. If you do that (and I think there is a good chance of America going before England in this matter), then we in England would be sending our ampere meters and volt meters to this country—to New York or Cambridge or Philadelphia, or Charleston, S. C., to have them verified. It would be quite worth while to send them across the Atlantic for that purpose if it could not be done at home.

There can be no doubt that the time is rapidly coming when house-to-house lighting, initiated about five years ago by Edison, will become

universal. I know the vast advantage of incandescent lighting in our own private houses, and I almost wonder that towns and cities have been so slow in adopting it. There can be no doubt that this heating influence of gas, which adds so much to the inconvenience of evening meetings, whether in the winter or in the summer, is to be superseded by the twenty-times cooler electric light. We are not now in the initiation stage, when electric companies have to demonstrate that they can do the thing, but we are stepping forward to the time when they are going to do it.

It is the object of a Conference of this kind to look ahead and to make arrangements for practical men to get the full benefits of science. Practical electricians desire all that science can do for them; it would be a waste of science if now that they begin to see the practicability of the electric light, and to make arrangements for it, we do not give them standards of measure. The number of these in connection with the electric light is a misfortune and a difficulty. But we see our way very much clearer by the results of this Conference to the promotion not only of a Central Bureau, but to the formation of offices and establishments in the chief towns in the United States in which any electrician may have his ampere meters or his volt meters tested within one-half per cent., which is enough for practical purposes.

I say nothing of units of resistance, for that is a settled thing. We know now how to measure resistances with very considerable accuracy, and it goes without saying that standards of resistance will be part of the work of the National Bureau.

I wish to express my cordial approval of the motion before us, and anything I can do to promote its being acted upon and carried into effect I would only be too glad to do.

Mr. PREECE. We in England are very much in the same position as you are in the United States upon this particular question. We are also placed very much in the same position as yourselves with respect to our Government. We are all sensitive when the Government interferes with our affairs in any way; but when we are in trouble or difficulty we are only too glad and anxious to call in its aid. Now, in this particular question our Government has certainly done as much as can be expected from it; but it has done it under pressure, and under such pressure as you now propose to bring against your own Government. Mr. Hilgard has told us as much as we know ourselves about the position the British Government takes in respect to weights and measures, viz, that we have a Department of State, where authorized standards are maintained, so that anybody can go there and compare his measure with the standards that are there kept; and he has also announced a fact which was altogether new to Sir William Thomson and myself, who both belong to a committee of the British Association, established some two or three years ago to induce the British Government to take part in this international work.

I am quite sure, after what we have heard to-day, that one of the first things we shall do on our return to England will be to try and force our Government into that which you propose to do, and which is essential, certainly, in order to prevent what Sir William Thomson has predicted would be carried out, and that is the sending over from England of our ampere meters and our volt meters to be tested by your inspectors. We think that that should be reversed, and that you should send your ampere meters and volt meters to us. There is absolutely a necessity for some Bureau of this kind. I have recently been conducting a series of very important experiments on the electric light. It was essential to me to have instruments which would be accurate and reliable, and although I tried instruments from every maker known to be fairly accurate, yet I scarcely succeeded in obtaining instruments that would record to an accuracy of 5 per cent. It was a common thing to find our ampere meter showing an error of even 10 per cent., but the volt meters were more accurate. At the present moment I do not know of a single instrument in England that can be bought from an instrument-maker that has recorded upon it a certain scale that can be relied upon to within 5 per cent. We must have in England, and you must have here, an office under the control of the Government, where such instruments can be kept for the purpose of verifying other instruments, as thermometers are verified by the Kew committee. Every nautical man can have his chronometer regulated at Greenwich and his barometers at Kew, and he ought to be able to have his ampere meters and his volt meters measured elsewhere, with the authoritative stamp of the Government upon them.

I sincerely hope that this meeting will agree upon a resolution recommending the establishment of such an office.

Professor E. J. HOUSTON. The age in which we live is one of practical applications of electricity—electric illumination, telegraphic and telephonic communication; and I think there is one idea in that connection that has not yet been thoroughly dwelt upon, which would show the very great necessity for the establishment of a National Bureau of Standards; and that is the effect of placing in an authoritative manner before the public what shall be the standards in making the general public conversant with them.

If you put this into the hands of the Government or a Government commission, and make a national thing of it, the result will be to familiarize the public with the meaning of words that are now in very common use, and which, I think, will in a few years become as common as "pounds" and "inches" are now. I think it is a matter of very great importance if the public are to use physical terms, or the terms to be employed in electricity, that they be taught to use them accurately.

Professor ABBE. We are accustomed in our chemical work to use thermometers, and we find great difficulty in reproducing Regnault's

thermometer. I doubt whether it can be done within half a millimeter. I think the thermometer lies at the basis of all our electrical work—either the mercurial or air thermometer. The matter is international as well as national, and our delegates to the International Bureau at Paris should be instructed to see to it that the International Bureau become one of physical standards as well as of those hitherto adopted by it—the electrical and other units.

So I should take pleasure in moving that this Conference of electricians assures the Commission which called us together that the establishment of a National and International Bureau of Physical Standards is of great importance.

Mr. N. S. KEITH. The time is coming, as Sir William Thomson has so happily suggested, when house-to-house lighting and utilization of electrical energy is to become extensive. Meters have been devised and will be devised for measuring this electrical energy, and this Conference should forcibly impress upon our Government the necessity of the establishment of a Bureau of Standards, which shall hereafter be utilized by the various cities in making a determination of meters which we shall be obliged to use. If those standards do not exist, it will be a daily source of trouble between the producer and the user, and cause innumerable lawsuits and disputes.

There are abundant disputes now as between the gas producer and the gas user, but the matter becomes simplified when it goes before the courts, because there has been a legalization of standards to a certain degree in the measurement of the material that gives light.

Professor WEAD. There are some things which ought to be said, although I do not like to appear to oppose what all would like to see. It cannot have escaped the knowledge of a great many that the Government has not always succeeded very well when it has tried to define what the specific standards are. For instance, it has said the weight of an imperial gallon of water shall be ten pounds and 277.278 cubic inches, whereas the actual volume is 277.127 cubic inches. It has attempted to define the gallon in two ways, and those two ways were found by a little more accurate measurement to be incompatible.

Two things have been involved in the discussion that we have heard: a Bureau of Standards and a Bureau of Research. There must be some research in connection with standards, but apparently this Bureau is intended to do new work.

Is that what we really desire to do? It has been a source of enjoyment to me during the past year to read over the testimony taken a few years ago in England before the Duke of Devonshire's commission on the advancement of science. And I was very much surprised to find how strong was the opposition of many of the gentlemen who testified before that commission against the establishment in England of such a Bureau of Research as seems to have been implied here in the words

that have been spoken by some of the gentlemen. They did not see how it could be made to work satisfactorily.

We have had set before us a beautiful ideal of a Central Bureau of Standards, which would furnish standards of the highest accuracy within easy reach of any one who wanted his instruments tested. We have that same scheme already embodied in our laws, if I mistake not—certainly the spirit of it—for the distribution of our standards of length, mass, and volume. All our States have been supplied with those standards; the intention was that counties should be supplied. I found one standard in a county in New York State in a room which was being used as a shoeshop. How many places are there in this country where anybody can get his standards compared? We have the same ideas before us now and they are not carried out. Do not let us be misled by high ideals.

One thing more about accuracy of standards. Perhaps all of us know how inaccurate gas standards may be. We do not expect of a gas meter the accuracy of ½ per cent., or 1 per cent. Unless I am misinformed, if a gas meter register 2 per cent. out of the way against the consumer, or 3 per cent. out of the way against the company, it is within the limits of accuracy, according to law. I cannot help noticing that the only gentleman who has spoken who has officially to do with the comparison of physical standards, fears that a Bureau for that purpose will be impracticable. I hope we will have this scheme more definitely set before us before we ask the Government to do for us what we may be better able to do for ourselves.

Professor ABBE. The motion before us merely declares that the National Conference expresses to the United States Electrical Commission its conviction that a National and an International Bureau of Physical Standards is of great importance. The means by which this is to be carried out can be considered or referred to a committee hereafter. I think everybody is in favor of having such a Bureau, and that is the very point which we wish to state.

Professor ROGERS. It seems to me a matter of supreme importance that this resolution shall express exactly what we mean. I therefore move that a committee of three be appointed to draft a resolution for the approval of the Conference.

Professor Wead spoke of the commission of the Duke of Devonshire, and the existence of the division of public opinion in England on the question of the endowment of research. There is no doubt that such a division does exist in the British public, and amongst scientific men, as to whether such endowment of research would be advisable. But I need hardly say that there is no difference of opinion among scientific men upon the importance of having a means of verifying our scientific standards. Every scientific man feels that he is very greatly indebted to the Kew committee for verifying thermometers, to the Greenwich Ob-

servatory for verifying chronometers, and we are glad also to have the existence of an organization at the Cavendish laboratory which stamps our ohms for us, if found correct, with the monogram of the Cavendish laboratory. I do not know whether the Johns Hopkins University has adopted a stamp for ohms tested under Professor Rowland's superintendence. If not, I do hope it will do so. Then, again, in case of tuning-forks, the whole scientific world accepts without question the monogram of Koenig. If anything happens to Koenig to whom should we look. What we want is a Bureau for verification much more than for research. I think it wise not to unnecessarily complicate the question of a bureau for verification with that much more debatable matter of the establishment of a Bureau for research.

Professor ABBE. The resolution as it now stands before us calls for the approval of a Bureau of Physical Standards. I think it should read "A Bureau for the Verification of Physical Standards, National and International."

Professor Rogers objected to the word "international," and again moved, by request, that a committee of three be appointed by the chair to draw up resolutions which shall be supposed to express the sentiments of the conference, to report to-morrow morning.

Professor MENDENHALL. In my own mind I am a little uncertain as to one point, and that is as to whether it is better for us to request the establishment of a separate Bureau for Physical Standards, or to ask for its incorporation into Bureaus already existing.

Professor FITZGERALD. I want to call the attention of those who are interested in the subject to a question which is gradually growing more and more important, namely, standards which are not usually called physical standards; for instance, the standards of pure milk and pure water. For the purpose of litigation it is very important that there should be some means by which you could get an authoritative statement as to whether a given sample of anything of water or milk is really pure or not. We are gradually coming to the fact that it is desirable to insist upon non-adulteration. In England and Ireland there are a certain number of analysts appointed, whose business it is to determine (for a small fee) whether a sample sent to them is pure and in accordance with the contract, just as it would be necessary to determine whether a certain quantity of electricity which is delivered to a house is in accordance with the contract. It is evidently very desirable that the same bureau and the same machinery by which one of these objects is accomplished should also accomplish the other object.

The question being upon Professors Rogers's motion, it was carried.

Moved and carried that the president of the Conference be added to the committee.

The president then announced the three members besides himself as follows: Professors Rogers, Newcomb, and Wead

Adjourned to 10 a. m. Thursday.

FOURTH DAY.

FRANKLIN INSTITUTE,
Thursday, September 11, 1884—10 o'clock a. m.

The Conference met pursuant to adjournment. The minutes for Tuesday, September 9, and Wednesday, September 10, being read, those for Wednesday were approved.

Professor TROWBRIDGE. Before proceeding to pass upon the minutes for Tuesday, I should like to recommend a change in the language of the motions. I move that the motions be so amended that the words "legal adoption by the United States" be substituted for the word "recognition" in the existing motions. One of the motions for example would then read: "That the Commission be authorized to take the proper steps to secure the legal adoption by the United States of the ohm defined by the Paris Conference as the legal unit of electrical resistance, in this country," and similarly for the other motions.

Professor CROSS. I would like to say one word. Quite a number of us who were discussing this matter before and after the adoption of the motion, felt that it would have been very desirable, in the first place, if the Paris Conference, instead of adopting these units absolutely, had adopted them provisionally in exactly the same way in which a certain length was provisionally adopted for the meter. That unit of length was actually afterwards changed, when a value more nearly approaching to the theoretical value of the meter was assigned to it. It would seem desirable, although departing from the custom of other countries, that we should put in the words "provisional adoption."

The motion, being submitted to the Conference for adoption, was carried.

The minutes of the Conference for Tuesday were then adopted as corrected.

The PRESIDENT. I will call upon the committee which was appointed to consider the subject of the watt, if it has any report to make.

Mr. PREECE. Now that it has been virtually decided what all electrical measurements shall be given in the terms of the C. G. S. system, it seems essential that all our units of work and power should also be connected with this system, so that we shall avoid the use of any coefficients whatever. Of all the units that have been introduced there really is not a single one more important than that which records the rate of doing work, whether electrical or mechanical. These two are precisely the same. It is quite possible to express the work done in an oil lamp, a gas lamp, or an arc lamp, in exactly the same terms; and, by way of parenthesis, I might just mention that a careful calculation of the work done in different systems of artificial illumination shows that while the simple sperm candle that is taken as the British standard of light consumes 97 watts, the gas lamp, such as we use in

London, burning 5 cubic feet per hour, giving, I think, 14 candles, consumes 62 watts per candle. And when we come to electric lighting we find that a good incandescent light gives out its illumination at the rate of 2.5 watts per candle. So you see how easy it is to express in terms of this watt the ratio of the energy expended in different modes of artificial illumination and to estimate the economy of different systems.

A watt is the amount of work per second which is done when an ampere current is maintained against the resistance of an ohm. This unit was proposed by Sir William Siemens two years ago, and was instantly adopted by all practical electricians in England. It has been used to a very large extent by practical electricians here. This unit was previously expressed as the volt-ampere. It is a subject of such vast importance that it only wants the authority of this Commission to add it to the lists of units already accepted. It has been accepted by the British Association at Montreal, where I brought before them the same proposition, and with the assistance of the British Association and this Commission, I have not the slightest doubt that the term "watt" will be introduced all over the world. I tried to bring it before the Paris Conference last year, and the reason why it was not accepted was twofold: first, the Germans objected to the introduction of any more English names, and, secondly, the French objected to it because they have no "w" in the French language. These two objections do not apply to us. Watt was an American as much as an Englishman, and we should be proud to apply his name to the unit which is connected with his measure.

This brings us to my second proposition. A unit of power was originally determined by Watt himself. By a series of very careful experiments, he measured the work done in a given time by a large number of horses, and the average work per horse turned out to be 22,000 foot pounds raised per minute. Somehow or other Watt thought that 22,000 foot pounds per minute was rather a small unit, so he added 50 per cent. to that, and called his "horse-power" 33,000 foot pounds per minute.

But that has nothing to do with us. We want a large unit for mechanical power. The watt is excessively valuable for electrical measurement, but it is too small for mechanical measurement. If we were to adopt the decimal system of measurement, we might call a thousand watts a kilo-watt. But there is no reason why horse-power should not be retained, and my proposition is (it is a proposition which is received with a great deal of favor by some of our mechanical engineers on the other side) that the horse-power itself should be altered. It is now 746 watts; but if we increase that by 34 per cent., that is, if we raise the horse-power from 33,000 foot pounds per minute to 44,000 foot pounds per minute, we shall obtain a horse-power which shall be exactly a thousand watts, and shall have a unit that shall connect all physical measurements with mechanical measurements, with no coeffi-

cient introduced, and we shall have a more sensible unit than we have had hitherto.

So the proposition that I intended for your acceptance is that the unit of power shall in future be the watt, and that the horse-power shall be raised 34 per cent. in order that it may be a thousand times the watt.

Professor NEWCOMB. I would like to ask a question with regard to the watt as defined by the electricians, in whose company I am not to be counted. It is defined purely electrically. I would like to ask, if it corresponds to the C. G. S. system, how many ergs per second is it?

Mr. PREECE. The watt is 10^7 ergs per second. The real unit of work in the C. G. S. system is an erg per second, but that is too small, and it has been raised to 10^7 to make it practical, which gives the watt.

Professor NEWCOMB. I would rather see the mechanical than the electrical definition adopted, although they amount to the same thing, and it would be better to take the sixth power of ten rather than the seventh power in our whole system of units. I make that suggestion to the members of the committee.

Professor CROSS. It seems to me that we might follow out Mr. Preece's suggestion, and call a thousand watts a kilo-watt, or we might call it a horse-power if there were no confusion with the French "force de cheval" and the English "horse-power" as so used.

The PRESIDENT. It has been suggested to me that no report has been called for from the Committee on the Standards of Light. The Committee on the Watt and the Standards of Light are the same.

Professor TROWBRIDGE. It is eminently proper that the two committees should be one. I am unable to get a meeting of the Committee on the Standards of Light, because several of the members have gone home. But I believe it is before you now in the resolution that you have adopted that this committee shall continue its labors, and submit a report in print within not less than three months.

Of course further experiments must be made, and the committee desires communications from those of the conferees who are interested in the subject, either by word or in writing.

The difficulties of the subject are very great. It seems to me the unit adopted by the French Conference is eminently an impracticable one. Mr. Preece has seen the experiments in Paris upon these (I might almost call) theoretical standards of light, and I think he agrees with me that America should not adopt them, at least hurriedly and before other experiments are made.

Mr. PREECE. This question of a proper standard of light is one that has occupied our attention in England, and there has existed a British Association Committee on Standards of Light now for two years, but which has not done much work. The work in connection with it has been entirely in the hands of Captain Abney. I do not think that there is any more careful experimenter or cautious physicist than Captain Abney in the two hemispheres. The question is still in doubt, and is one that requires very careful and not very hurried consideration. Professor

George Forbes is a member of that committee. During the next four months we shall undertake to make considerable experiments on this subject in England, and I trust this meeting will form a permanent committee, so that simultaneously with our work in England experiments may be carried on on this side of the water.

There are two points involved: (1) A practical one of the establishment of some standard of light that we can all understand and reproduce in our own laboratories at any time; (2) a more difficult one of a standard of white light. We want a practical standard of light to enable us to carry out measurements in terms of something we know to be correct. We want also to have a definition of white light so as to be able to express the efficiency and the value of different characters of light in terms of something that shall collect all the lights together by some such system as we have been discussing.

We propose during the next twelve months to put aside the question of white light, and examine all the practical standards that can be found in the market that are in use.

We are also going to test the value of the human eye to measure and compare these different lights. There is unquestionably a personal equation connected with this measurement of light. It has never been thoroughly gone into on our side of the water, and I do not think it has been on this. One of our first acts when we go back to London will be to organize a system of measuring the efficiency of the human eye for the determination of the value of light.

At the present moment there are three or four real standards. In England we have the British standard; the candle that has been defined by act of Parliament. It is a particular kind of sperm candle, that burns a hundred and twenty grains every hour and gives a flame of a certain height. Then there is, in Germany, another candle different in value from this English candle. In France there is the Carcel lamp, burning a certain quantity of oil per hour and giving a flame of a certain height. The value of the French standard is actually 9.7 that of the British candle. Then there is another standard that I have produced that has not been accepted at present by anybody but myself. It is not a standard of light, it is a standard of illumination. I want to know the illumination at different places of a street that is lighted by gas or the electric light. For instance, the illumination of a shop-front, of the sides of a house. We want to have some mode of expressing that, in order to understand the intensity of light quite independently of the source of light. The standard I have proposed is the light upon a surface such as white paper when the light is emitted by a British candle at 12.7 inches, which corresponds with the light emitted by a French Carcel lamp at a distance of a meter.

There are a few questions which we shall discuss through this committee in England: (1) The most practicable standard of light; (2) the practical standard of illumination; and (3) the most practical mode of measuring photometrically these two.

The British candle is an admirable theoretical standard for expressing the numerical value of light, but we find the incandescent lamp is by far the most convenient instrument to compare lights by. It is not a very difficult thing to determine the light emitted by an incandescent lamp with a given current, and we can reproduce that light at any time after the experiment when we know our current. An incandescent lamp for measuring light can be maintained constant for any number of months. Professor Abney and myself have worked very hard on this question, and we find an incandescent lamp is a very handy and convenient instrument for measuring other lights, and we can express this light in terms of the British candle so as to bring in the unit.

I shall not be able to take part in the consideration of this matter by the committee. I do not think we should be hasty. I think it will take months to inquire into it. However, before I leave for England, which will be in the middle of October, I will put in writing all my views and submit them to the committee for their consideration.

I might add just one word more. Another object that we have in view is that by using an incandescent lamp whose light depends upon the work expended in the given filament, we use a standard whose light can be expressed in terms of the watt, and through the watt we come at once to the measurement of light in the C. G. S. system of absolute units.

Mr. KOYL. Would it not be better to use, instead of an incandescent lamp which is composed of a long filament of carbon, something having a square surface? The light emitted by a platinum surface of half a centimeter, or a centimeter, which has definite resistance under a given current, would be constant. We would then measure at right angles to the illuminating surface, and have a much more constant standard of light than with the carbon filament, where we measure at right angles to the curve of the carbon filament.

Professor CROSS. I would like to say that there appears no necessity to use carbon light as a primary standard; but as a secondary standard I can see that it would be one of the best that we can have. I do not understand whether Mr. Preece intends to use the incandescent light as a primary or as a secondary standard.

Mr. PREECE. At present we only comtemplate using the incandescent light as a secondary standard. With regard to the use of a platinum plate, that was proposed by Henry Draper. It was found that platinum was most unreliable under those circumstances. There was a constant disintegration at the surface, and the light varied a great deal. I do not think any experiments with platinum are valuable.

Professor CROSS. It seems to me that we ought not to attempt to go too far; that is, to connect the sensation of light, which is of course all we mean when we say "light," with the C. G. S. system. All we know is that in a lamp so arranged we have a definite amount of energy that affects the eye. Light proper is a sensation, and of course we cannot hope to connect that with the C. G. S. system.

The PRESIDENT. I do not understand that there is any motion before the house on this subject, and I will now proceed with the discussion of "The Theory of the Dynamo-Electric Machine." I only claim in the skeleton of the theory which I have here prepared to give a few points which may be of interest and possibly of value to those who are constructing these machines.

The principal losses of the machine I put down under the following heads: (1) Mechanical friction; (2) Foucault currents in the armature; (3) energy of the current used in sustaining the magnet; (4) self-induction of the coils; (5) heating of the armature.

Of course the efficiency of the machine would be equal to the whole work of the machine minus the different losses divided by the work, namely:

$$E = \frac{w - l_1 - l_2 - etc.}{w}$$

Thus, when the losses are known, the efficiency of the machine is known.

The mechanical friction I shall not discuss.

With respect to Foucault currents in the armature, by dividing up the armature in the proper way, we can get rid of most of these. It is very often effected in the Siemens armature by dividing up the armature into discs.

I have purposely omitted the loss due to change of magnetism in the armature as the armature revolves. I drew attention to this fact several years ago. It has been recently experimented upon and found that, although there is some heating effect, it is very small indeed.

With respect to the energy used in sustaining the magnet, if the magnet were of steel there would, of course, be no loss. The only reason for not using a steel magnet is that the field is comparatively weak. The field of a steel magnet is, I suppose, less than one-third of the field due to a good electro magnet; the two could not be made equal by any possible means. Therefore, in most dynamo machines, the magnet is produced by the current.

It is a question what the form of the magnet and the position of these coils should be in order to get the greatest field with the least expenditure of energy. I have one or two propositions to make on this subject which I think are of some interest.

The first proposition I have to make is that a round magnet is better than one of elongated cross-section. If the coils are long, and they are usually long enough for the purpose, although the theory assumes an infinite length, the magnetic force at any time acting on a round iron core is exactly the same as on an elongated core. But the area of a circular section is much greater than that of an elongated section of the same circumference, and therefore the same amount of wire which would be used to go around the elongated magnet, would, if extended on a circular section of the same circumference, surround much more iron.

The principal object of making an elongated magnet is that it may in-

clude the whole length of the armature. Most makers who adopt this form think it better to elongate the cross-section than to have a long pole piece. But we have seen that the round form is more efficient in general than the elongated form, and the only question is whether it will be more efficient in this particular case. I shall proceed in this theory upon the known fact that we can consider lines of force as if they were conducted by the iron and the air outside. The conductivity of the iron for the lines of force is very great, much greater than that of air. I experimented on it many years ago, and my idea is that it varies (according to the degree of magnetization) from several hundred up to 5,000 times that of air. The conductivity for iron is very great, especially for wrought iron; for cast iron it is probably less. Therefore the lines of force will be conducted down through the iron from any point over a circular cross section very nearly as easily as they are from an elongated cross-section, and the saving in the wire will be considerable.

I have another proposition to make with respect to the magnet, and that is that one circuit of the lines of force is better than a number. There is a loss from having a number of electro-magnets, even if they are round. For this reason, that the same magnetic force is acting in each of these coils provided there is the same number of wires per unit of length; and the same wire will go more times around the same iron concentrated in one magnet than when subdivided into several, and will, therefore, act upon it with more magnetizing force.

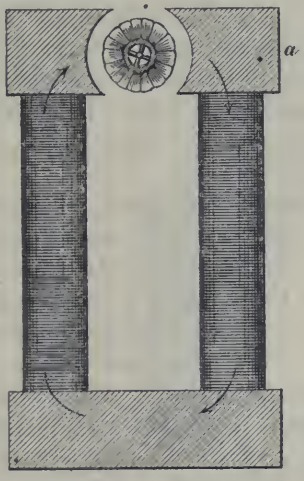


FIG. 1.

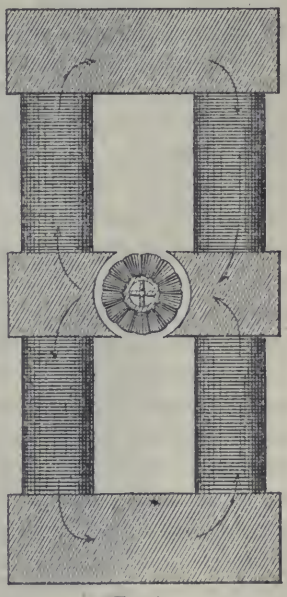


FIG 2.

That proposition not only applies to this form of magnet (Fig. 1), but it also applies to the form where we have the armature revolving between two magnets like this (Fig. 2), because we can turn this lower

magnet over and bring the two together. The circuits of the lines of force are around in this direction and in this (arrows, Fig. 2). So that there are two circuits of the lines of force instead of one. The energy expended for a given amount of work will be less with this form (Fig. 1) than with this (Fig. 2). That is of very great value to makers of machines.

The theorem applies to a number of those old machines where there was a very large number of little magnets revolving around other little magnets. More work is used in sustaining the magnets in that form of machine than in the more modern form where we have only a few circuits.

I had a number of drawings made of magnets in the Electrical Exhibition, and I find very great difference in this respect; more difference where Siemens armatures are used than in any other kind. In discussing these drawings I do not give any names, nor say whether one machine as a whole is better or worse than another.

First, I will discuss the general forms of the magnet, and then I wish to say something in respect to the form of the pole pieces that inclose the armature. Of course this form belongs both to the Gramme ring and the Siemens armature. Most modern machines are of this nature, either Gramme or Siemens, and we may consider them both one if we wish.

We will now proceed with respect to the field in this form of magnet (Fig. 3). The lines of force proceed down the magnet, and are supposed to go across here (*a b*), where wires wound around the revolving armature cut them, and so produce a current. It is evident that any lines which escape across this open space (arrows) are lost. If there was any leakage of the wire around the magnet, the current, instead of going around the magnet, would go off somewhere else, and we should consider the machine defective because there was a loss of the current. So if any of these lines of force, instead of going directly across there (*a b*), go across the open space (arrows), as they naturally would do, all those lines of force are lost, and we would have to add so much more current in order to make up for this outside loss. I have an illustration of such losses of lines of force from a drawing, which I will give you (Fig. 4).

This machine has two magnets—one above and one below. The lines of force pass up through here (*a b c d*) and then out and around through here (*e e*), &c., to complete the circuit. As I saw the machine in the exhibition these outside pieces (*e e*) were closer to the poles of the magnets than I have drawn them. If they are put too near, some lines of force, instead of passing across the field of force, where the wires revolve, as they ought to do, pass off at these openings, the circuits going around in this way (arrows *f f*). In this case there is a loss due to leakage of the lines of force, and we shall therefore have to expend more energy in keeping up the magnet. There is energy expended in keeping up the field outside as well as in keeping up the field through the armature. It is important that this point should be considered. These questions, "How many lines of force go across this opening and are effective in

producing the current, and how many escape off without passing through this opening and are lost?" are just as important as the question of the leakage of the current in the wire. There are defects in many of these machines in that respect. In this form of machine (Fig. 1), where there is a simple circuit, this magnet has to be attached some-

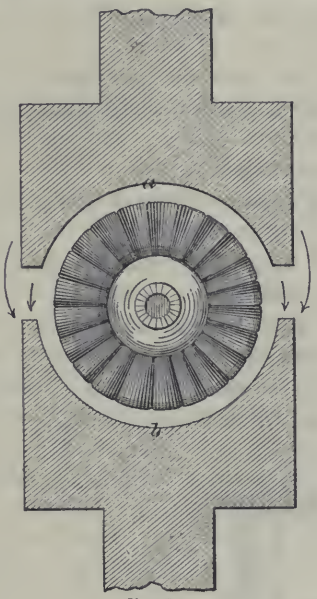


FIG. 3.

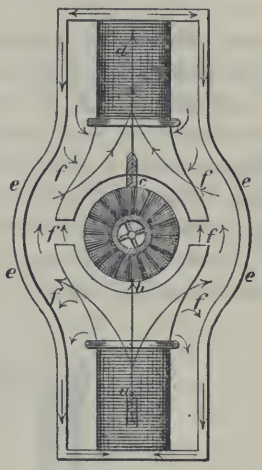


FIG. 4.

where. Very often the magnet is turned vertically, poles downward, and attached to a cast-iron bench. I have no doubt that some lines of force are lost (not much perhaps) in passing across from the magnet to this iron bench. The makers of the machine, I suppose, considered this to some extent, but what is needed is measurement on that point.

Here is another form of magnet (Fig 5). That machine would be de-

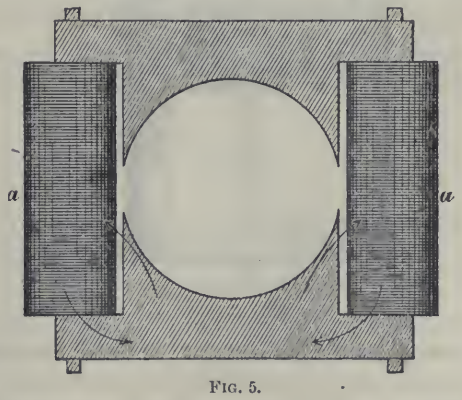


FIG. 5.

fective. It has two magnets and two magnetic circuits in the place of one, and many of the lines of force probably make little private cir-

cuits of their own around in that way (arrows). Those lines of force are of course lost, and it is more or less defective in that respect. It would be better to diminish the number of magnetic circuits to one.

Mr. ELIHU THOMSON. I should like to ask where are the cores of the field magnet in that figure, for I happen to be connected with a machine which that represents very closely, and there is also another machine which is figured by that.

Professor ROWLAND. As I understand, *a a* are the cores. There would be a little more opening at the four corners perhaps than I have drawn.

Mr. ELIHU THOMSON. Thank you; that satisfies me.

Professor ROWLAND. I am only giving a general idea of the principle of these machines, and I do not refer to any in particular.

It is also important that these lines of magnetic induction shall find easy passage around in order to produce the most intense field. Thus the opening between the armature and pole pieces must be made as small as possible, in order that the lines of force may find easy passage across it. Everybody recognizes that. Suppose we had a machine made in the following manner (Fig. 6), in which there is a magnet with

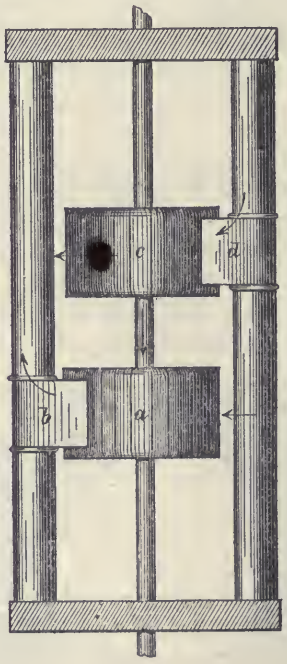


FIG. 6.

a Gramme ring here (*a*), and pole piece here (*b*), a ring here (*c*), and pole piece here (*d*), but no pole pieces opposite these. How are the lines of force to pass around? I do not know that it would be easy to see how. They evidently go around here (arrows) and get to the other

side the best way they can. There is no easy passage around for the lines of force in this case.

A MEMBER. May they not to some extent follow the shaft?

Professor ROWLAND. It is evident that if the shaft is made large enough some go along the shaft in that way (arrow), but there is no easy way for them to get around.

I have here a formula for the amount of work which one has to expend upon a magnet in order to produce a certain effect. I will take the case which I have considered most efficient, where there is one magnetic circuit. It is an original idea of Faraday that these lines of force are conducted. We suppose the lines of force to pass through the iron and across the opening in this way (arrows, Fig. 1), and they are caused to do that by what may be called the magneto-motive force of the helix.

I will just obtain an expression for the number of lines of force B. This is not the quantity which Maxwell considers, but it includes the whole number of lines of force which pass through the magnet. We may write B, proportional to N, the number of turns of the wire around the magnet, and C, the current; and inversely proportional to the resistance to these lines of force in going around the circuit. The resistance to the lines of force is proportional to L, the length of the iron of the system, divided by S, the cross-section of the magnet, supposing it to be uniform, into μ, the magnetic permeability of the iron (or the conductivity of the iron for the lines of force). This quantity μ varies with the current, and can readily be obtained. Some years ago I gave a formula for it. It can be expressed simply as dependent upon the magnetization of the iron and a constant depending upon the iron alone. We have something more to add:

Let l be twice the width of the opening between armature and pole piece, and A the area across which the lines of force flow; then we have to add $\frac{l}{A}$ and another quantity, which we can call p, which depends upon the resistance of these lines of force which escape in all directions and represents the loss due to that escapement. Thus we have the final value for the number of lines of force (or rather induction) in the magnet

$$B=\frac{N\,C}{\frac{L}{S\mu}+\frac{l}{A+p}}$$

This gives us an equation which may be solved with respect to μ. The curve for the magnetic permeability is of this nature (Fig. 7). It will be of a more or less flat form, according to the value of l and p. Therefore, in increasing the magnetic force upon the magnet, it becomes easier and easier to magnetize it until a certain point is reached, and after that it becomes harder and harder. In practice the core should have sufficient cross-section to produce a very strong magnetic field, but not so great as to require too much wire to wind it. The two must

be balanced, which can only be done by calculation or, better, by experiments on the machine. By examining the force of the magnet at each point, and in that way getting an idea of how these lines of force go, we can see whether the cross-section of the core is large enough to produce all the lines of force necessary for our purpose or not. Of course, in order to have sufficient magneto-motive force to send lines of force across the opening in sufficient quantity, we must have sufficient wire. As the thickness of the coil is increased, we have to use more wire in proportion for a certain diameter of core, which is a disadvantage, since each coil acts very nearly the same as every other in producing force. But if the core is very short indeed, wire must be piled on it to

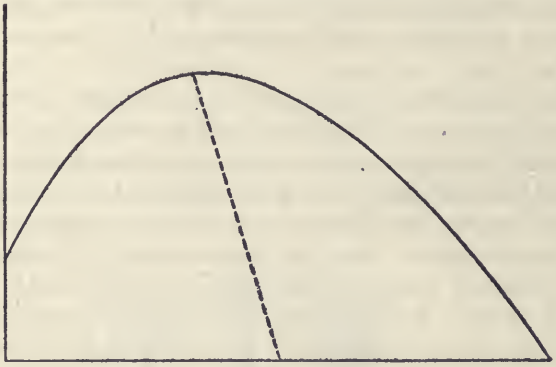


FIG. 7.

a very great extent in order to get sufficient magneto-motive force, and as iron is cheaper than copper it might be better to lengthen out the core. I do not know where the lengthening should end, but I should suppose when the requisite wire on the magnet makes a moderately thin layer. Of course, as we lengthen out the magnet, the resistance of the circuit to magnetization becomes greater; but that is a very small quantity. I do not suppose the increase is very much for a considerable lengthening of the magnet. As I said before, the magnetic conductivity of iron is many times greater than that of air, and we can lengthen out the cores without producing much loss on account of that lengthening.

Some persons have suggested that there might be a slight gain from the fact that iron, after it has been magnetized a great number of times in the same direction, rather likes to be magnetized in the same direction afterwards. If the core is made of any material similar to steel, such as wrought iron or anything of that sort, it might be possible to have some gain from the coercive power of the magnet. There would be loss from that cause at first; but from the continual use of the machine I think it very likely the iron might get a set in the direction of the force. If the core were of steel, for instance, it might be that one could send a strong current through at first and magnetize the steel, and then be able to diminish the current considerably and still keep up a very large magneto-motive force. I do not know how practical that would be, but it seems to me that one could produce a very strong field in that way.

In the commencement of the operation of the machine, we would have to send a powerful current to magnetize the steel, and then, without stopping the current, to diminish it. Then the set of the steel would be in the same direction with the current and produce the field with less expenditure of energy than if it were simply iron.

There is no difference between a shunt and a series machine. The magnetizing force on the magnet I have set down as proportional to the number of turns multiplied by the current; that is, proportional to the cross-section of the coils multiplied by the current per unit of cross-section, so that the magnetizing action can be the same either from a strong current or a weak current. Therefore, if the exterior dimensions of the coils are the same in both cases, the same energy is expended in each in order to produce the same force, so that there is no difference between a shunt machine and a series machine as far as the economy of the magnet is concerned.

I do not wish to take up too much of your time, and will go on to the heating of the armature. Of course the amount of energy expended in the heating of the armature will be dependent on the resistance of the armature. It is well known that the efficiency of the circuit will merely depend upon the relation between the resistance of the armature and the exterior circuit.

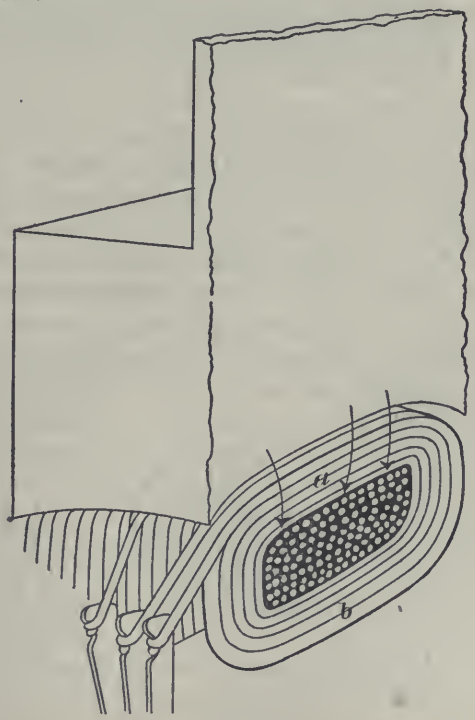


FIG. 8.

There is one other point in regard to losses; "dead wire," I think, is the technical term for it; I mean that portion of the wire which does

not cut the lines of force. In the Gramme pattern the armature is inside of the rings. In the Siemens pattern the coils are around the ends of the armature. In a section of the Gramme ring (Fig. 8), the outside portion of the wire (*a*) is active, since the lines of force follow the core and the outside of the ring around; but the lines of force do not go through the core of the ring, so that the inside portion (*b*) is dead, so that we can say nearly half the wire is dead wire. In the Siemens ar-

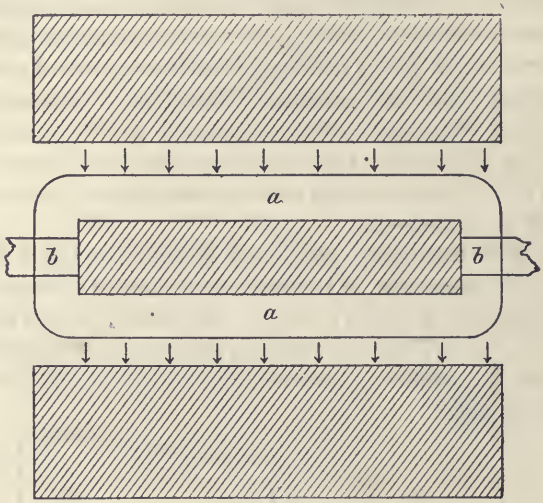


FIG. 9.

mature one cannot see immediately how much dead wire there will be, because it depends upon the length of the armature. The wire is wound around in that way (Fig. 9), and this portion (*a a*) is active, and this portion (*b b*) is dead. If the armature is very thick we would have more dead wire than when it is simply long. I cannot say which has the more dead wire, but I dare say the Gramme has more than the Siemens. Fur-

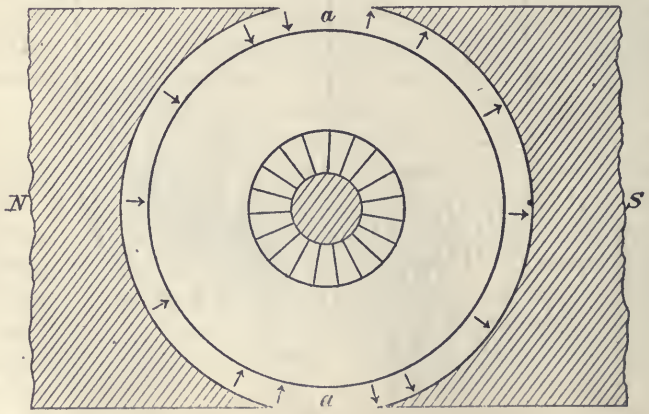


FIG. 10.

theremore, either in the Gramme ring or the Siemens armature (Fig. 10) we have the lines of force running across here (arrows); that portion is

active; but these portions (*a a*) in between the poles are dead, and when the armature revolves we have the lines of force turning around, and I think that would add more dead wire. I believe an attempt has been made to throw out these coils.

There is no necessity to go further. As I have said, the efficiency of the circuit depends upon the ratio of the resistance of the armature to the resistance of the wires, and therefore, as far as this point is concerned, any machine can be made as efficient as one pleases by putting in greater and greater external resistance. But as the magnet remains the same, we would find a point where the efficiency as a whole would not increase for an increase of external resistance, but would actually diminish. There are other things to be taken account of, such as losses due to the self-induction of the coils which produce sparks in them. I have requested Professor Fitzgerald to take up that point, and will leave it for him to consider.

There is another point with regard to the dynamo which can be treated in this simple manner with no use of the calculus. This is very simple reasoning if you only know the principles. I shall consider two machines similar in all respects, except that one is larger than the other, or rather consider one machine, and see what the effect will be when that machine gradually changes in size.

The point from which we start shall be that the magnetic field is constant in the two machines. For, owing to the fact that there is a limit in the magnetization of a magnet, we cannot have a field with more than certain strength produced by iron, and I will suppose that the strength is reasonably near that maximum for iron. It cannot be up to the maximum strength, of course, but somewhere near it. I made some experiments many years ago upon an ordinary magnet, the results of which were published in Silliman's Journal, by means of what I call the magnetic proof plane. (Am. J. Sci., vol. 10, 1875, p. 14.) It applies beautifully to dynamo machines, and I obtained everything with it that I have referred to here. If I remember right, I found in that magnet about one-third of the field that an iron magnet could possibly have.

It is theoretically possible to get a force equal to the magnetizability of the iron, but practically, I suppose that instance is about the case of the ordinary dynamo machine. We start, then, with the supposition that the field of force in the two machines, one of which is larger than the other, is constant. That is to say, the magnetizing force at any point of one machine is equal to that at a similar point in the other machine. In making a drawing of the machines, it would not matter about the scale of dimensions; the force at a certain point is a certain amount whatever the scale.

Next consider what must be the current through the wire in the two machines. There are the same numbers of turns of wire around the magnet, and everything is the same except the dimensions. Consider the current passing around the coil of a tangent galvanometer. If the

galvanometer grow, in order to produce the same effect at the center (and not only at the center but at every point), the current must increase in direct proportion to the radius of the coil. When the coil is twice as large the current must be twice as large, in order to produce the same force at every point. Thus, if there is no difference in the material of the two machines, we have their currents in direct proportion to their linear dimensions. Make a machine twice as large and the current in the coils must be twice as great to produce the same magneto-motive force. Of course the wire has increased in size; if the machine has increased to twice its original size the cross-section of the wire has increased four times. In other words, from that cause the current per unit of area will vary inversely as the square of l, the linear dimensions; and since we have found the current to vary directly as l, in order to retain the same force in the field, by a combination of the two results, it varies inversely, as l. Therefore, so far as the magnets are concerned, the heating effect, which depends upon the current per unit of cross-section, will decrease with the size, while the surface will increase in proportion to the square of the size. There will, therefore, be less danger of heating in a large magnet than in a small magnet, but this is only with respect to the magnet.

The resistance of any part of the machine varies, of course, directly as the length of the wire, and inversely as the cross-section. The cross-section varies as l^2, so that resistance varies inversely as l. Therefore the larger the machine the less the resistance; one machine being twice as large as the other, the resistance will be half as great. This applies not only to the work of the magnets, but to the work of the armature.

I will now consider the electro-motive force. The electro-motive force is proportional to the product of the current and the resistance, or we may write $E=RC$. We have the current proportional to l, and the resistance inversely proportional to l; therefore the electro-motive force is constant. As we are running the machine, it turns out that the electro-motive force does not vary with the size, but we shall presently see how this is modified so as to get greater electro-motive force for the larger machine.

The work done is $C^2 R$ in any part of the machine, or in the whole machine, just as you please. This varies directly as l. Therefore the one machine which is twice as large as the other requires twice as much power to run it, and twice as much electrical energy comes out of it. But it is to be remembered that the weight of the machine varies as l^3, and we only get work proportional to l out of it.

So far as results go, we have constructed two machines which differ only in size. The efficiency of these two machines is a constant quantity. That will be rather startling to some, who think a large machine is more efficient than a small one. As far as we have gone in any two machines, one of which is simply larger than the other, the efficiency is the same.

But if we calculate the angular velocity of the armature to keep the proper current we shall find that it varies inversely as the square of the linear dimensions. In other words, in one machine twice as large a as another the velocity of the armature must be only one-fourth as great in order to produce the proper current in the wires. This takes account, I think, of every irregularity in the machine. The two machines are exactly the same in every respect. I have not added the loss for the self-induction of the coil. I have anidea that this also should be taken into account, but Mr. Fitzgerald will consider that point.

Now the question comes up, can we increase the velocity of the armature above that point? Is it practically necessary that we should run one machine at one-fourth of the angular velocity if it is twice as large? It is a practical question; but I should certainly think the velocity was not in that proportion. I should think it would be more nearly inversely as the size and not inversely as the square of the size. If so, then by so arranging the wire of the armature as to increase the proportion of external resistance we can have the same current per unit of section when running the armature faster and the same electro-motive force. If we do that, this whole theory applies; but we shall have increased the external resistance of the machine in comparison with the resistance of the armature, and when we do that we increase the efficiency of the machine.

I think it is from this cause that we find large machines more efficient than smaller ones; but it is also evident that there is a limit to this, which can only be obtained, I suppose, from practically making the machines and seeing how much faster they may be run without flying to pieces. As far as this theory goes, the increase comes not from the size of the machine, but from the fact that we can get a greater electro-motive force with the same angular velocity, and so can reduce the internal resistance in proportion. In very large machines we can make the wire with one turn, not several turns—simply bars on the machines. We thus decrease the resistance of the machine, and at the same time, if we run it above this proportion which I have pointed out, we obtain the proper electro-motive force. In other words, the proper electro-motive force is more easily obtained from the large than the small machine, because it is not practically necessary to decrease the velocity so as to keep it inversely as the square of the size.

FRANCIS J. FITZGERALD. The point which I was led to call attention to some years ago, and which has been investigated and experimented upon by several, is this: Taking a coil in which the current is going in one direction, as soon as the coil passes across the brush the current is reversed. That in itself necessitates no loss, if the energy of the stopping current could be utilized in starting it again in the opposite direction; but what I called attention to was that in general a coil is short-circuited when passing from one of its positions to the other across the

brushes. Consequently the energy of stopping is used in sending sudden currents through the coil, due to its own self-induction.*

The loss of energy due to this cause will be proportional to the self-induction of the coil and to the square of the current passing through it. If L is the self-induction of each coil and C the current passing through it, LC^2 is the loss of energy due to the self-induction of the coil being wasted on the local current. This will be for each coil; hence, if n were the number of coils that were passed per second, nLC^2 would be proportional to the loss per second owing to this effect.†

This can be put into another form by substituting for nL in terms of the self-induction per unit of length of the ring. If v is the velocity of the edge of the ring and l is the self-induction per unit of length of the ring, then necessarily $vl = n$L; and vlC^2 will represent the loss of power owing to the self-induction of the coils.

It was a very difficult thing to experiment upon this effect as to whether there was any loss of power due to the self-induction of the ring. The term vl is of the nature of a small resistance, which we may call R, so that the loss may be written RC^2. R is a resistance of the machine due to self-induction, and is proportional to the linear velocity of the ring and the self-induction per unit of length. The apparent resistance of the ring may be easily obtained by rotating it under the action of the earth's magnetic force. This theory is due to the Messrs. Ayrton & Perry and to M. Joubert, the former of whom made an important series of experiments on a Gramme ring rotated under the action of the earth's magnetic field, so as to make sure of the effects being calculable and not

*Some doubt was expressed by Mr. Thomson as to whether this was necessary in a Gramme armature, but it seems pretty clear that if the current in the internal circuit is to be continuous the brush must be in contact with at least two sections of the armature at once, and that during this time the corresponding coil will be short-circuited through the brush, and that, moreover, it is during this interval that the current which had been running through this coil ceases to do so, and that it would cease instantaneously only for the self-induction of the coil, which keeps the current running until its energy is wasted in heating the coil by this local current. The new current is not started in this coil until its terminal begins to leave the brush, and at this instant, its self-induction being opposed to that of the rest of the circuit, the *el. m. f.* is generally sufficient to produce a spark wherein more energy is wasted. The self-induction opposing the starting of the current, except in so far as it produces this spark, diminishes the current, but is not a source of loss of energy. If the brush do not short-circuit each coil, *i. e.*, if it never touches more than one section of the armature at a time, the whole current in the armature field magnets and internal circuit will be stopped between each passage of a section of the armature, and the enormous self-induction introduced would produce destructive sparking. It might, however, be worth trying whether all sparking could not be entirely prevented by introducing a condenser to accumulate the electricity in the same manner as in Ruhmkorff's coil.—F.

†Professor Silvanus Thompson said subsequently that it was equal to one-fourth nL_1i^2, where i was the total current running from the brushes; but it seems doubtful whether all the energy of self-induction is wasted on the local current and whether some of it does not go to help the internal current; and, if so, all that can be asserted is that the power wasted is *proportional* to nLC^2.—F.

affected by the uncertain reaction of the magnetism of the armature on that in the pole pieces of any ordinary magnet.* It was found that there was increase of resistance, owing to an increase of velocity. The increase was a little larger than the calculations, but that might be accounted for by the impossibility of preventing the jumping of the brush upon the commutator in rapidly turning a ring. The result of the experiments thus led to the conclusion that the increase of resistance was to be calculated upon, and that it was well not to run dynamos faster than a certain velocity, because this resistance varied as the linear velocity of the ring.

I may say something of the effect of size to which Professor Rowland has called your attention. In the first place (taking Professor Rowland's construction), the angular velocity varies inversely as the square of the linear dimensions. Therefore the velocity of the edge of the ring varies inversely as the linear dimensions; secondly, the effect of the self-induction per unit of length of the ring will be independent of the size of the machine, for the coefficient of self-induction is in general proportional to the linear dimensions of the system, and therefore the self-induction per unit of length will be independent of the dimensions of the system. These combined show that the resistance R is inversely proportional to the linear dimensions; and inasmuch as the current varies as the linear dimensions, the loss of power RC^2 will be directly proportional to the linear dimensions. But the whole power (as Professor Rowland has shown) is directly proportional to the linear dimensions, so that the proportion of the power lost to the whole power will be constant and independent of the size of the machine.

It is possible by constructing what are called "uni-polar" dynamos to get over this loss, owing to the self-induction. For instance, take the simplest case of Faraday's experiment with a magnet rotating. In that case there is evidently no sudden change in the current, and there will be no loss, so far as I can see. We will have the self-induction of the diminishing current helping to increase the starting current, and there will be no loss due to the stopping and starting. Some people find difficulty in understanding how there may be no loss in changing the direction of a force. It is the case of a crank running upon a wheel; there is no loss owing to the reciprocation of the crank, unless it is a bad crank, which joggles upon its bearings. The loss due to short circuits would be like the loss owing to the pressure of a bad crank upon its bearings. But in the case of the uni-polar machine it is like a crank in which the motion goes on uniformly without any unnecessary joggle round and round.

It is possible, also, to arrange Gramme armatures in such a way that

* Professor Silvanus Thompson called attention to what these gentlemen first pointed out, that as l is the self-induction per unit of length of the ring the waste is independent of the number of sections into which the ring is divided. It seems doubtful, however, whether this is strictly true of the power wasted in sparks.—F.

there will be no loss owing to this cause; that is, by arranging a number of the coils in parallel circuit instead of in series, with a number of different brushes, and the current divided between these brushes. It is evident that it is possible to arrange the brushes in a certain position, so that they shall make contact with a coil at the time when the potential at the ends is the same as the potential of the brushes. There will then be no tendency for the current to start or stop through the armature, and therefore it must start or stop gradually. On the other hand, it is evidently possible to arrange the electro-motive force so that there will be some instant in which there is no current running in this coil. If that is the moment which is chosen for breaking the current there will be no spark owing to the self-induction at that time. So that by making the contact between the brushes and the corresponding coil at the time when the potential of the brushes is the same as that of the ends of the coil, and when there is no current running in that coil, we will necessarily have a machine in which there is no loss in this way owing to the sudden stopping of the current.

I would just like to say something upon another point in connection with the question that Professor Rowland mentioned. In a large dynamo we have the obvious advantage that it is possible to make the clearance very much less in proportion than in a small dynamo. We can thus make the distance between the armature and the poles very much smaller in proportion in the large dynamo than in the small one. Another advantage in the large dynamo is that, on account of the electro-motive force being nearly the same in the two dynamos, we may make the proportion between the area involved in insulating material and the space occupied by the wire smaller in the large dynamo, thereby producing an additional increased efficiency.

With reference to stress of centrifugal force, while I was waiting I worked out that point. I have come to the conclusion that the tension of the material per unit of area will vary directly as the square of the linear velocity, and that in Professor Rowland's case, the linear velocity being inversely proportional to the linear dimensions, the tension per unit of area will vary inversely as the square of the linear dimensions, and that, therefore, the larger Gramme ring may be driven considerably faster without any danger of its flying to pieces. I think the tension of the ring is exactly proportional to the square of the linear velocity; but I am not sure.

Mr. ELIHU THOMSON. I have been at work on dynamo machines for a long time, and my experience teaches me that some of the statements brought forward are not correct. For example, there is a factor that has been omitted in regard to the extension of poles of magnets. We have a figure showing an extended pole piece, under which an armature is revolved, and it is stated that the lines of force find it easy to go a certain way. I do not think so. We find on examining the machine during its motion that the armature itself is a magnet of an opposing

character; that it produces poles which are opposing the magnetism of the field poles, and, if the armatures are of uniform size and section throughout, that the magnetism developed here in opposition to the field magnetism will be the same as here [indicating]; consequently you have an opposing force to the field magnet at this point greater than it would be there [indicating], and consequently you have produced a loss of magnetic force.

Another fact, and this is what may happen very readily, there is a field running up the side of the magnet and turning in again. By the extension of the poles, carrying your magnet along here, you will obviate to some extent that difficulty.

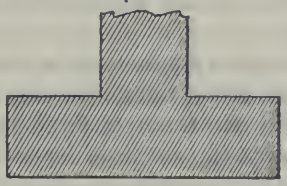


FIG. 1a.

Then we had the statement made that one set of magnets is better than two opposing sets, as in certain well-known forms of machines. I would like to take exception to that again, and for very simple reasons. Take one magnet (Fig. 1), and you give every chance for that kind of wasting action up the side, which, taking into consideration the fact that the armatures are opposing the magnetism of the field magnet when it is revolving, is extremely simple to see. Place another magnet here (Fig. 2), and you force the magnetic lines to go through the armature, and you consequently have a much better machine. That leaves out of consideration the fact that you may have a small plate of material here (Fig. 1a) of only two and one-half or three inches thick in proportion to the magnet which has enormous reaching power, if you may choose to call it so. Taking into consideration again the enormous currents that are developed in that armature opposing the magnetism of the field through the armature, and you see what a shortening of the circuit of the armature is made when the armature is put in any sort of proximity to the ends of the field poles.

Again, take the form of the machine where we have the field magnets arranged upon pole pieces, and we bring the iron down to the side to close the circuit of magnetization in that manner (Fig. 4). The question arises (I have not investigated the subject fully) whether this loss by short circuit will be made up in part by the external magnetism of the coils upon this iron near them. In other words, whether by putting the iron out here, not too far away, and again not too near, and gradually increasing the distance, you do not get an external magnetism by the iron lying close up to the exterior of the coil, which would make up for part of this loss of short-circuiting by the proximity of the field poles.

With regard to the use of long or short magnets in the dynamo, if we take into consideration what I have just stated in regard to the magnetizing power of the armature, it will depend upon the diameter of the armature and the length of the magnetic conductor that is in the armature. Assuming the armature to be made up of radial bars, or even disks, the length of magnet you will have to employ will depend upon the opposing magnetic effect of the armature and, I should say, the distance between the field magnet and the armature. If you wish to get reaching power or depth of field, in order to reach any distance through the armature, the iron being too far away, you will have to use long magnets. If you have an armature which has iron projections which will make a close contact, you can use much shorter magnets with equal results.

In regard to the use of steel magnets, it is well known that steel in no condition can accept as much magnetism as soft iron, and it is also well known that if you harden the exterior of a magnet with a soft-iron interior that the radiation to the soft-iron interior of the magnetic lines will cause a short circuit of magnetism in the interior of the magnet. So, I do not think we can look for any improvement over soft iron for field magnets. There are many other considerations that might be noticed that show economy lies in using soft iron only where we wish to get the greatest available magnetic strength.

I cannot agree either in the statement that there is dead wire in the Gramme armature or in any other armature that incloses a magnetic circuit. If that were the case, then all wires upon bobbins would be dead wire. We could not produce a dynamo machine which had bobbins in it; for what is the Gramme ring but a series of bobbins, which, when they pass the field poles, are magnetized there just as they are in the old Holmes and Alliance machine? They pass from that position of saturation where they include the greatest number of lines of force to a position of less saturation, and over on the other side to a position where the saturation is reversed. What suggests the reason that the outside wire would be more efficient than the inside wire is simply because, as I look at it, there is a tendency here that is unrestrained of the armature to kill itself while it is passing at this point—a tendency of the armature to magnetize the magnet and close the circuit around it inside of the ring, which will diminish the effect of it on this wire. However, this closing of the circuit on the outside of the core causes the field magnet to lose control of the direction of lines of force. This closure of the circuit inside the ring, however, is simple and easy to take place, because here the iron is practically removed from the powerful control of the field magnet and the coils are where they can act as magnetic coils, and close their own circuits. How can we increase the efficiency of such a machine? Evidently by preventing that closure by putting some material there to prevent that control from leaving the lines of force inside as well as outside. There is an effect in such machines

that has not been called attention to, and that is that we cannot take the same sizes of wire and with a larger machine expect to realize the theoretical efficiency. I hold, however, that theory, when it is true and complete, always coincides with true and complete practice; it has no intermediate stopping point. If you take into consideration every variable, you are bound to arrive at the complete practice; you cannot have one thing theoretical and the other practical. It has been proved mathematically, and it is very easy to go over the ground, but I will not take your attention now, that if we use the same sizes of wire on armature and field we have the power of the machine increasing as the fourth power of its linear dimensions. But if we attempt to apply that in practice we find this, that the surface of the machine only increases as the square of the dimensions, whereas the cubic capacity—in other words, its power to retain heat—increases as the cube of its dimensions. The result of that would be that in a large machine we will have to provide a greater amount proportionately of cooling surface or enlarge the size of the wire. The power of the dynamo run for series lighting (I may say I have only determined it approximately) increases about as the cube of its linear dimensions, instead of the fourth power—I have stated that incorrectly; it is as the cube instead of as the fourth power of the linear dimensions.

In the study of the Gramme armature and its method of commutation, I think we have yet to learn a great deal. I cannot see any reason why in the Gramme armature during the commutation there should be any loss at all. Let us figure the ring and study the action of commutation. A coil goes upon a commutator, passes it, and comes upon another, and so on. I hold that if the period of contact of these brushes upon these segments is properly arranged there will be no loss; in other words, the time of contact that must exist between these two segments and the brush will be the time that it takes for the coil to lose its current, and the commutation must not occur at any other time than when the coil is ready to lose the current, and contact must be made until it acquires current in the opposite direction of the same amount it is to take when the break shall be made. I do not know that I have made that perfectly clear, but I will go over it again. If the contact remains for a sufficient period to allow the coil to lose its current, and the commutation is only being made while it is running down in electro-motive force and continues until the current rises on the other side until it is fit to go into the circuit—in other words, until it just conducts the current that the other side demands—then you will have no loss from commutation. Make it just at that period, and where is there any loss?

In regard to the resistance of armature coils, I think there is a fact that is generally omitted in considering dynamo machines. It is this: that we cannot consider the resistance of the armature at any time as the working resistance. We cannot measure it in any position and let that be taken as the resistance of the armature coil when it is working.

Why? Because in any dynamo that furnishes us continuous currents we will have this condition sometimes existing: In the Gramme we have two series of coils, in the Brush two series of coils put in multiple arc for a time; and if you measure the machine while not at work, you take it for granted that the current when the machine is running will be equal in those two branches. Will it ever be equal in those two branches? The energy developed by that armature will depend upon whether that current divides equally for the time being. In other words, whether the ring is equally balanced, whether the brushes are at equal temperature, whether the armature is put at the center of the field. The wire will become more energetic when it is nearer the field pole, and the effect will be that you are sure to displace your commutation brushes. More current will pass through one branch than through the other, and you will have more heat developed in one than in the other, and you will not have a perfect dynamo. You will have more energy consumed in that dynamo; and there is not a machine, I may say, that I do not regard as getting in that condition. The resistance of the machine when still will not be the same as when running, because the currents do not divide as in still resistance.

That is about all I have to say in regard to the subject under discussion, except that, if I have an opportunity, I should like to call attention to a number of other matters that affect the working of dynamos, that have been derived from studying dynamos and making very many types of machines and experimenting with them, my whole time being almost entirely devoted to the study of dynamos.

Adjourned until 3 o'clock p. m.

AFTERNOON SESSION.

THURSDAY, *September* 11—3 p. m.

Professor GIBBS in the chair.

Professor ROWLAND. With respect to Mr. Thomson's remarks, I am very glad to see the matter taken up in this spirit and to have my principles intelligently criticized. However, there was one remark which I wish to state immediately as an error, of course, with regard to the steel. Steel can be magnetized to exactly the same degree as soft iron. There is no difference between soft iron and steel in that respect, except that we require an immensely greater force to magnetize steel to the same extent as iron. There are some old papers of mine, which were published in the "Philosophical Magazine," I believe, in 1873, relating to experiments where I took iron and steel and several other metals, and showed that the maximum magnetization was the same in all cases.

But with respect to a number of statements with regard to flat magnets and round magnets I am very glad to see my remarks criticised in the manner that they were, because it shows the need of exactly what I stated; and that is experiments upon this subject. The question is one of quantity. My reasoning gave results in one direction, and Mr.

Thomson gave reasons for making the magnet in another way, and it is a quantitative question of course as to which is the best; and for that reason I want very much to see experiments made in the manner which I have described by means of this "magnetic proof plane," so as to find out what the escape of the lines of magnetic force in all cases is.

I think we can decide on one point that was brought up without any trouble, and that is with respect to the dynamo made with extended pole piece (Fig. 2), where it was assumed that the lines of force had a tendency to go in a particular direction, that it was a sort of gun shooting the lines of force through the armature. That is not true, because they do not have any tendency to go that way at all, and we would only add that much to the area of the end of the magnet. Very few lines of force will go out there, and by putting this additional magnet on we add to the area of the magnet. The lines of force will go out at the sides probably in greater numbers than they would at the end, so that I do not think that particular objection holds in that particular case. It is a question of quantity; the thing should be measured and found out. I see very plainly in my own mind that more lines of force would go out the side by adding this iron here (Fig. 2) than would go out at the end of it by leaving it vacant, as in Fig. 1. But it is a matter of mere opinion. Another reason for having fewer magnets is that the surface is greater in the case of the larger number than of the smaller number for the lines of force to escape from.

There was another point brought up here with respect to the machine which was made in this way (Fig. 4). It was stated that there was some gain from the magnetic action of this coil on the iron outside. There is undoubtedly a gain: the question is how much, and whether more lines do not escape than would make up for that. With no experiments to go on, it is a case of judgment. My own judgment would be that there would be very little gain; but, as I said before, the thing should be measured, and then we could find out about that point.

Mr. KEITH. I have drawn upon the board diagrams, by which I may illustrate the evolution of a machine that is at the exhibition; and I will begin by the following description: Suppose Fig. 11 to represent

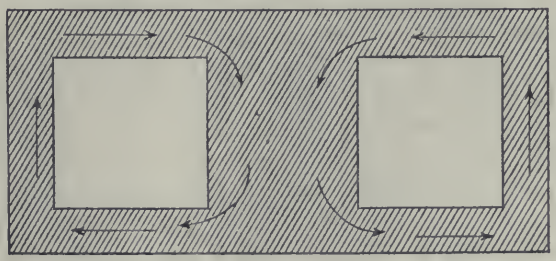


FIG. 11.

two closed magnetic circuits, placed so that their polar ends are in juxtaposition; then there is no external induction; the magnetic circuits

are complete. The magnetic current lines may be said to circulate in the direction of the arrows. The magnetic potential, then, if the mass be properly distributed, is the same at all points.

Now, according to the theory, which is very well accepted, if we cross any of these magnetic current lines by wires we produce in these wires currents of electricity flowing steadily during constant motion. As soon as we make a space here (A, Fig. 12), in order to produce evidence

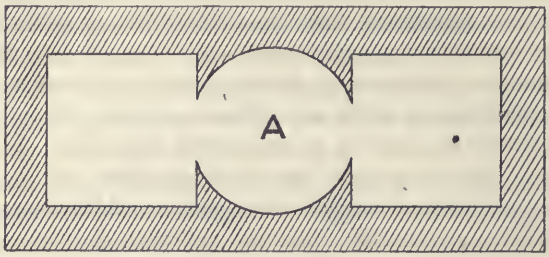


FIG. 12.

of these lines of force, we introduce greater resistance to the magnetic lines, and the tendency is for a majority of the lines to cross from pole to pole, but others distribute themselves in various curves according to well-known laws. If we take this mass of metal and place it back again, and if it exactly fills the space, we by this act again close the magnetic circuit, and there will be no external indication of the field. Of course, it will not be possible to pass wires between such surfaces; but by taking out the core and turning it so as to make it smaller, to admit of the passage of the wire completely around the core in this space (B, Fig. 13), we will then obtain the highest possible efficiency of field under that construction of the magnet and armature. If we then take this core and cut it transversely into a mechanically-desirable number of disks, and insulate them to prevent the formation of Foucault currents, we will then have a practical magnetic core, upon which is wound a wire of dimensions that fill the space between the core and the poles so constituted; and if this armature be rotated, the wire being connected with the proper collector, we will have the highest possible concentration of the magnetic force of these two currents.

Of course, mechanically, we cannot bring these surfaces into contact. We must allow room for the movements to take place, so we will let that space B be as narrow as possible. The result is, then, the highest mechanically-possible efficiency of the machine for the production of electric current. Furthermore, the greatest concentration of magnetic lines of force on this armature is upon the diametrically-opposite points, as shown by the line *x y* (Fig. 13). When this armature is rotated the currents are generated in the coils which surround it. The currents are taken off at points 90 degrees removed from the line *x y*, as at points *a b*. The tendency of these currents is to magnetize the armature so that the poles of the armature will be 90 degrees from the poles which

are induced in them. If the space is considerable, and there is not enough magnetic material of the core of the armature within the coils which surround it to make a complete closure of the magnetic circuit, there will be a distortion of the poles generated. In other words, they will be moved forward by reason of the counteracting forces of the magnetic induction and the electro-magnetic effect upon the core of the armature; but if this armature has been made as nearly mechanically perfect as possible, and the current which plays around it does not exercise a dynamic effect upon the core sufficient to distort the magnetic poles, there will be no necessity, under the varying circumstances to which dynamo machines of constant potential are submitted to move the brushes in the direction of the rotation.

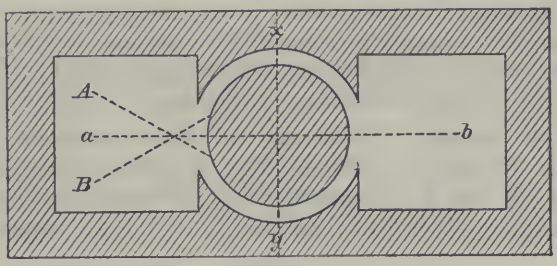


FIG. 13.

This desideratum is obtained, it strikes me, by the machine referred to and with a very high degree of efficiency on that point. As practice always best illustrates a theory, I will state that I have seen one of these machines capable of supplying 200 incandescent lamps. One of the lamps being placed from binding post to binding post, 199 of them were switched off instantly. The remaining one at the binding posts, as nearly as I can form judgment by the eye (the others being out of range of the eye, so as to avoid an error of judgment on that point), was not raised more than 5 per cent. in illuminating value. These machines show very nearly constant potential. There is no necessity for removal of the brushes to compensate for varying resistances in the external circuit.

It seems to me that a machine so constructed satisfies as nearly as mechanically possible the theory which seems to be most generally accepted, and that there are various points which might be stated about this machine as illustrating some of the points which have been brought before this meeting. One of them relates to the association of two magnetic circuits, producing what we call consequent poles. I understand (though I cannot speak from knowledge on this point, but I believe it,) that when two magnetic circuits are placed in this relation the tendency, or the result, is to concentrate the lines of force upon the true magnetic line, or the true line of division of these magnetic influences. Certainly

the practical facts point to this—that where only one magnet is used there is, under the laws governing the location of poles of magnets and distribution of magnetic lines, an entirely different field from that which exists under an association of magnets. There is a greater tendency to increase the magnetic strength of the points, or horns, as will be found by magnetic exploration. It may be that if the armature be proportioned in all respects to the field after the manner I have here shown that there will be a difference in the distribution of lines of force. They certainly will approximate better to the theoretical points than they do in some machines now at the exhibition.

Professor SILVANUS THOMPSON. I have a few criticisms and remarks to make upon some of the methods that have been brought before us this morning. In the first place I wish to refer to the matter which Professor Fitzgerald brought before us of the apparent or spurious resistance caused in a rotating armature by the self-induction of the armature coils. The way in which I have looked at the matter is slightly different from that of Professor Fitzgerald. I want to correct his formulæ in one respect. If we want to treat the resistance in the path of the circuit we must remember, letting n be the number of revolutions per second, and L the coefficient of self-induction of the armature entirely around, and writing i instead of C for the current, we have $n\frac{Li^2}{4}$.

Professor FITZGERALD (interrupting). Will Professor Thompson remember that I spoke of the resistance as "varying as" not as "equal to" in the formula I gave.

Professor THOMPSON. I want to be able to apply Ohm's law and write that the current is equal to so and so, whatever it may be. I will write it down. I think it is right.

$$i=\frac{E}{R+\frac{n L i^2}{4}}$$

Professors Edmund and Barret, when they published this matter on electro-motors, actually said, if you make the experiment and calculate this out you can express the self-induction in ohms. I could not help thinking, when I saw that, that there was some mistake in calling a coefficient a thing which could be expressed in ohms. I find that self-induction is a length; and in absolute dimensions resistance is a velocity. There must be something more than what Professors Edmund and Barrett wrote. The fact is if we cross out the i^2, we shall have the true apparent self-induction. Suppose we write E for the electro-motive force and i for the current. Then E may be written as the sum of a number of other things. The work of the current is measured among other things by the work done in overcoming the mere statical resistance. To that

we have to add the work done in overcoming the work of building up the intrinsic energy of the current, or its self-induction, $Li\frac{di}{dt}$. To that we must add the work done in increasing the current in the field $\left(n\frac{di}{dt}\right)$ and to that we have to add $\left(i\frac{dn}{dt}\right)$ if the field varies. Neglecting these last terms, which relate to the external field, which we may suppose to be constant, we see that this term might be written along with that one as a spurious resistance. If we take an integration for any length of time, and in this case we simply cross out our d's, we shall have virtually $\frac{Li^2}{t}$; that is to say, the self-induction, divided by the time, may be in the nature of a resistance that is virtually a velocity.

If we have the value of the spurious resistance for any velocity we can measure it for any other velocity, and therefore we can make a correction for different speeds of driving our machine, knowing what it is for any given velocity. L is the coefficient of self-induction for the whole armature, and it makes no difference for the value of the n how many sections the armature is divided into. I will not call attention to the effect on self-induction of dividing up the armature into several sections.

Now comes in an important point where the actual waste comes in. It is not in the fact that the self-induction sets up for a moment a spurious resistance; that makes it go like a smaller machine. The waste comes in when the coil is short-circuited. In an ordinary commutator of the Gramme or Siemens type that coil is short-circuited, and it is then no part of the circuit of the dynamo. It is a neighboring circuit, and we have no right to talk about self-induction; we must consider the current apart from the separate circuit, which is closed in itself. It is there that the waste occurs. The current is running around this little circuit in the coil and heat is produced, and I hold that it is absolutely wrong to put two brushes at the top, one in advance of the other, to kill the spark, because you prolong the motion of the coil during which this deleterious action comes into play. You may notice the spark which is caused by the local current running around from one brush into the other through the coil that is short-circuited.

Again, if we pass from this subject to the more weighty consideration of how the self-induction term is related to the machine as a whole, we may write for the electro-motive force of the machine $E=4\,nAH$. That is the form in which I have written the equation of dynamos in a book, which contains a theory of the dynamo. The number of revolutions per second is n. I have used this letter instead of α, the angular velocity, because I thought it was more suitable to have the number of revolutions than the radians per second. A is the effective equivalent area of the armature with many or few turns, and H is the average value of

the field in which this armature is supposed to be rotating. Running the machine very fast so as to get quite a large electro-motive force will increase the quantity n that comes into that spurious resistance, and increase the self-induction and the spark troubles. Increasing the area of the coils increases the self-induction, which increases with the square of the number of coils; so that if you want to run at a high electro-motive force you must not run at a high speed and not have any turns on your armature. The only way in which you can do that is to have a very strong field, and I believe that the true progress of the dynamo for the next twelve months to come will be in making the fields of our dynamo so immensely strong that we shall not have to have very many turns of wire nor have to run at an outrageously high speed. If we succeed in getting strong fields, we need not have so many turns and we can run at a low speed, which is eventually better. We gain immensely by this, because there are other things besides sparks that come into play when we have high speed, by reason of the complicated brushes. There is the question of lead, for example, at the brushes, which was alluded to by Mr. Keith.

Now we come to the remarks of the president when he opened this discussion. He spoke about the shapes of magnets. I have made the remark in my lectures that circular cross-section should be the best because a circle has the greatest area with the least perimeter. But in practice for very large machines a circular form is not the very best form. If we take a circular cross-section for our pillar of iron, the iron at the middle is so far removed from the coils outside that it does not, at any rate so soon, get magnetized as the iron at the outside, and, what is worse, the magnetism actually turns around inside and short-circuits itself through the interior. If you will take tubes of iron, one outside of the other, and then magnetize them with a coil on the outside, you will succeed in magnetizing the outside one, and some of the inner ones; but the innermost ones are not only not magnetized, but you may have them magnetized in the reverse direction in some cases. I have tried it. [Applause.] Again, we find if we take a very thick bar of iron and magnetize it by means of a coil and examine how the magnetism increases when we increase the current and plot out a curve showing the relation between the two, we do not in general get that which we ought to get if the magnetic permeability were what it is always found by high theorists to be. Nor are we concerned with the permeability of the middle body of the long bar; but what we are concerned with is the permeability of the outer surface of the bar running out to the end of it in the place where the armatures revolve. The magnetism of iron in the so-called unsaturated state is proportional to the number of turns of the wire on the magnet multiplied by the current running through it, and its curve runs up straight and then turns around and our magnetism begins to be got only at a higher price. By doubling the current you no longer get double the magnetism. When

everything is saturated it can be magnetized no more than ice can. The figures which Professor Rowland mentioned were something like a thousand or two thousand. This has been surpassed by an experiment of Professor Ewing in Scotland, who gives us figures like five, ten, or fifteen thousand. But such numbers do not express in the least degree the increase; we want to express the permeability of our magnet as a whole. It is nothing like that; it is more like twenty or thirty thousand we get. It is true as to the permeability of a bar of iron that you may be able to stuff magnetism into it from end to end, some of it leaking out at the sides and coming out of the end. You may magnetize it through and through until that central body shall be magnetized to that extent, the permeability being five thousand to the square centimeter, or something like that; but you do not get that from the bar when the armature is being revolved.

I was talking with Professor Hughes concerning experiments on the multiplying effects upon a bar of iron of putting it into a coil. The experiment was tried something in the way of Rowland's method, but the results were not the same. He took a coil and put a core inside of it, and laid it upon a board which had a magnetic needle at a certain distance. He had then an arrangement to find out the magnetic effect on this needle. He tried both iron and steel cores. When he put in an iron core of the softest Swedish iron, he found that it did not magnify the effect on the bar more than eight times. He did not mean that the permeability of the iron was only eight, but that the permeability of that circuit as a whole was only eight. The circuit was a complex one. He considered the whole effect of the air, and the piece of copper wire, and the permeability of that circuit taken, as a whole, was only eight, instead of five thousand or twenty thousand. We want a great deal more of magnetic experimenting on circuits of which only a part consists of iron. We must deal in a practical way with this matter.

There is another matter that has not been dwelt upon—the effect upon the magnetic energies of the iron, due to the grain of the iron. I believe we are only beginning to realize the importance of employing wrought iron instead of cast iron in the building of dynamos. I might mention that Mr. Crompton, of England, who employs a dynamo of that type which has been so beautifully abused here to-day, has succeeded—without any other change except merely substituting for cast iron, wrought iron, chosen so that the grains should run in a proper direction, having the same machine, the same armature, the same number of coils, and everything alike except the qualities of the iron—in getting an increase of 100 per cent. in the circuit of his dynamo; that is, it worked as a machine of twice the capacity. He doubled the power of his machine by merely changing the quality of the iron. We all know it is easier to magnetize a bar of iron according to its length than in any other way; that is, to magnetize it along the grain. Try an experiment on a piece of wrought iron in which the grain can be observed,

or notice in what way it runs as it comes from the rolling-mill, and you will find it far easier to magnetize it along the grain than across the grain. You will get a greater amount of magnetization from that piece of iron so magnetized. Try it, and you will find there is an enormous importance in this question of the grain. If we magnetize the iron so that our lines of magnetic force run with the grain of the iron, we shall get a higher degree of magnetism inside, a greater value for H, and our electromotive force is proportional to that quantity. We ought so to arrange our dynamos that the lines of force should emerge from the iron into the air at the points where there is end grain in the iron. I should like to try the effect of constructing a dynamo with a double or single pole at the top where the iron is so turned that the grain points in each to the place where the magnetic force was to be. I believe it would give a better effect.

In this book to which I referred I deduce a large number of equations for different types of dynamos, and I am going to trouble you with two simple ones, because they bear upon this question of the energy and quantity of iron in the dynamo. I am going to write out the two expressions I obtained for the electro-motive force of a series and a shunt form of machine, respectively. The letters are the same in each case, except that S is used for the number of turns in the series machine and Z for the number of turns of wire wound on the shunt. R is the resistance of the armature; n is the number of revolutions per second; A is the same as before; G always goes along with K and is a purely geometrical coefficient, depending upon the shape and size of the iron masses of the machine. K is the magnetic permeability, what Professor Rowland has designated by μ.

Before I explain I will write down the two equations, the first for the series machine, and the second for the shunt machine.

$$E = \frac{1}{\sigma}\left(4n\,AGK - \frac{R}{S}\right)$$

$$E = \frac{1}{\sigma}\left(\frac{4n\,AGK - r_s r_a + R(r_s + r_a)}{RZ}\right)$$

The resistance of the shunt is denoted by r_s; the resistance of the armature by r_a. You will notice that in the second equation we have virtually the conductivity of the machine as a whole in the numerator of the fraction, instead of the resistance.

Now this factor $\frac{1}{\sigma}$ comes in both equations. I have taken σ to represent the permeability, and I am going to assume that the permeability does not rise in this kind of construction as it may do in the middle of the piece of pure iron. Practically it is constant for a time in respect to the whole circuit, and then gradually diminishes. The value of that permeability, when the magnetizing current is varied, dimin-

ishes from its original value, and the law of its change may be represented by $\frac{K}{2 + Si.}$ I do not say this is the true law, but it is the one that practically meets the facts of the case most beautifully. Only if the current increases and becomes very large indeed does this second term of the denominator become of value. If we multiply this by G, a geometrical coefficient, and by S, the number of turns, and by i, the current, that quantity may be held to represent the average field. It is proportional to the permeability depending upon this quantity σ.

What is this quantity σ? In examining what that means, let us plot out once more the curve of electro-motive force, neglecting the inductive effects. We get a curve which rises and then turns over. (Fig. 14.)

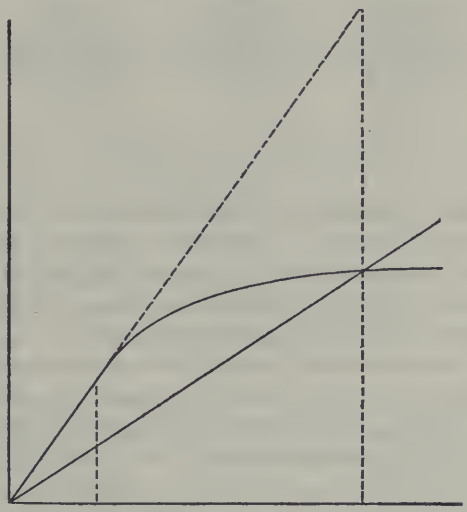


FIG. 14.

If you will find out what σ means, it means this: If that iron never got saturated, the magnetism would go on increasing and stop at a straight line, but it diminishes, and there comes a time when the magnetism is one-half what it ought to have been if the iron had remained unsaturated. When you have got to that point you have a very large number of turns. σ is the reciprocal of that number of ampère turns, which, for all practical purposes, will reduce the iron back to one-half its permeability. If, therefore, we have very good iron indeed, and plenty of it, so that it takes a tremendous number of turns, then we shall have a very large electro-motive force when such iron is used in the dynamo. It is most important to note that the effective power is practically proportional to that quantity which reduces the magnetic susceptibility of the iron. With this criticism that this theoretical curve (Fig. 7) true as it is for its own circuit, is not true for such circuits as we have to deal with in dynamos, I shall dismiss that point.

I have more than once criticised the use of multiple magnets or poles. There are some machines I forbear to mention, in which there is an immense block of iron used for a pole piece, then one or two other pillars of iron separately magnetized with a block to contain them on the other side. Those are scientifically bad, not only for the reason Professor Rowland has named, that it takes more wire to run around than one solid piece, but for another reason. Let us look at the figure (Fig. 15). If these are the south poles (A A A) the currents from the first pole will run down (arrow) and from the second one will run up (arrow); and in between the two pole pieces these currents are absolutely hin-

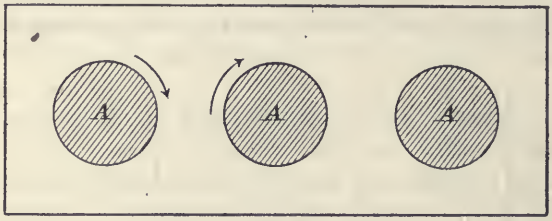


FIG. 15.

dering one another's magnetism, and it would be far better to wipe out the currents there altogether. Therefore, we gain in work by running the two into one, because we are not employing part of our current to destroy what another part is doing where these turns come in there between two pieces of iron in opposite directions.

We ought not to forget that mechanical considerations are quite as important as electrical ones. Take the case which our president put upon the board. He seemed to think that a field magnet was far better as it was, without doing another thing to it. I do not agree with him. In the first place this structure (Fig. 1) is a big piece of iron up in the air, and if you put a bed-plate there you short-circuit some of the currents in the bed-plate. If you turn it up in this direction (upside down)—I believe it was in that way our president drew it, because I was struck with the very beautiful indication it gave of the leakage. I do not like that arrangement of having great masses of iron vibrating like a tuning-fork up in the air. There is no reason because you support the bed-plate at the two ends that you cannot have a third piece. It is not symmetrical bilaterally with this axis when you have got the other side to the magnet. These two corners might magnetize as strong as these two [indicating], and you will have to cut them away in one place or the other in order to have an equality of the field bilaterally with respect to the line I have drawn.

We have another drawing of a machine, in which I quite agree with the criticism passed by our president, and it struck me as being really a remarkable thing that no exception had ever been taken to this machine before. It is a machine which had one pole-piece above and one below, and two Gramme rings, one here and the other there in between

(Fig. 6). I have heard that called, I believe, a unipolar machine. It strikes me that it has two poles. I cannot help thinking that that machine is altogether bad; it may work; the fact is, it is hard to have a dynamo machine with a Gramme ring in it that does not work. But I have examined this machine in one respect; I tried to find out what was occurring in the vicinity of the armature and piece of coil, where we would naturally expect another pole piece, and I found this: the very current which was running through the armature and over the brushes to the commutator was including a pole in the armature, and that was inducing a false pole in the piece of iron opposite. Therefore, by no means all the lines of force that ought to go across there did go across; they spread out into the air. I examined this machine once in England, and I saw a similar machine in your exhibition, and it seemed to me as a striking commentary upon it that over that machine was a sign which ran, "Do not approach this dynamo, for fear of magnetizing your watches."

I do not think that this curve of magnetizability represents the whole truth, because in our dynamos we cannot deal with iron at one constant temperature, and the magnetizability of iron changes so much with the temperature that no dynamo can be considered at all complete that does not take that into account.

I want to refer to another curious point which no one has publicly stated anywhere, and of which no explanation has been given. Take a Gramme dynamo; these here, say, are the pole pieces (Fig. 16), and it is

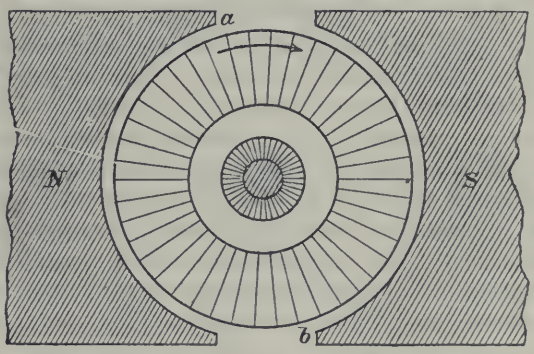


Fig. 16.

run in this direction (arrow). If no current were being taken from the machine you would have induced poles; if there were currents taken up from the machine to be used there is a resultant north and a resultant south (N. S.); they are the effective poles of the machine in action. I have shown elsewhere that the ratio of the two fields due to the field of magnetization and due to the current is proportional to the sine of the leaning.

Now there is another thing I want to refer to. In a Gramme machine,

which has been running for some time, the ordinary effect is that that corner and that corner (*a* and *b*) get hot, and the other corners are comparatively cold. In order to explain that curious fact observed by Cabanet, when I was last in Paris, he told me, and he is able now to verify the fact, that he took one of these beautiful laboratory Gramme machines, with the magnet like a horseshoe, and used that as a motor. After a while he found the field magnets going down in strength. After having spoiled it in that way he used it as a field magnet or generator, and he found the magnet coming up again to very nearly the original value. I thought it was impossible; the fields respectively due to the field magnet and the armature current absolutely help one another in a motor; and in a dynamo they oppose one another. I do not know how much they hinder one another in a dynamo, but to some extent the lines of force cross. There would be no electrical work done as work in the circuit if they did not. How can we explain that the helping field of the motor absolutely demagnetizes the magnet, and the working field of a dynamo absolutely helps the field magnets? The one observation I made which connected these two is the following: I have made a magnet 20 per cent. stronger by putting the armature on very slowly and pulling it off suddenly. Reverse the operation and pull it off slowly and the magnetism will go down. I have made magnets diminish 10 or 20 per cent. in this way. This explains both these effects. If you take away a piece of iron from a magnet, you do work against the magnetic attraction, and the induced currents which are set up in the iron or steel of that magnet are always in such a direction as to oppose the motion; that is to say, they make the magnet pull more strongly than before. In the reverse motion there are induced currents which occur when you allow the armature to slam up, which are such as to oppose the motion of slamming and decrease the magnetism. You can magnetize more highly by suddenly pulling off and putting on quietly. That is precisely what is occurring in the field here when this machine is used as a generator (Fig. 18). At (*a*) and (*b*) you are pulling off the armature, and you get heat because of the induced currents. This explains also how, when used as a motor, the magnet is weakened, and when used as a generator the magnet is strengthened.

Professor ROWLAND. I have listened with great interest to this discussion by Professor Thompson. I have only one point. I am the defender of two theorems with respect to the machine, and that is with respect to round magnets and the simpler circuit. With respect to the round magnets, I may state that I was very much astonished—indeed, I may say, astounded—at the remarks of Professor Thompson, that the magnetic force in the center of the round core was not the same in direction as at the surface. Indeed, it is so revolutionary that I should imagine that it would produce such an effect on the scientific world as would a demonstration that the law of conservation of energy was not correct, or that gravitation did not vary inversely as the square of the

distance. Therefore I must strongly dissent from it. I do not think the matter can be discussed. If anything further is said on it it must be in the way of a scientific paper, which will take up the subject and make a grand sensation in the scientific world.

Professor SILVANUS THOMPSON. My reasons are, that I had taken concentric tubes and put them inside each other and then magnetized them and found that the magnetism of the inner ones was reversed; so that if my experiment showed anything it showed that the internal pieces of iron were reversed in magnetization.

Mr. KEITH. I desire to say that I tried the same experiment, and published the fact, not as my own determination, but in answer to an inquiry in the Electrical World some three or four months ago; not claiming it as my discovery, but simply as an observation that had been made.

Professor ROWLAND. It only requires a moment to see what the real cause of this is. It is perfectly evident. Suppose you had a tube like that, with an internal piece like that. You magnetize with a coil on the outside. The lines of force have got to go through to the central piece here. In the outside piece they have a complete circuit around, so that when the experiment is carried out in this way with concentric tubes, of course the outside piece will be magnetized more than the center.

Professor FORBES. I venture to say that even with concentric tubes the magnetism of the central tube could not be the reverse of the outer tube. From what Professor Thompson has said I think I know the whole difficulty. He has magnetized his tube and then taken away the magnetizing force, and afterwards tested it. It is perfectly well known that magnetism may be reversed in that case. De la Rue showed that in 1836; it is in his book. But as long as you do not take away the magnetizing force it is not reversed.

Professor ELIHU THOMSON. I remember reading an observation which bears upon this subject; I think it is by a German physicist. He states that where you use a coil of large diameter and put a short piece of steel in the interior, that, singular to state, that piece of steel becomes permanently magnetized in an opposite direction from what you would suppose. Whether it is so or not, I do not know. I thought at the time that perhaps the explanation might be found in this manner: that treating the magnetic field as a closed circuit of lines of force, the very great density of magnetic force in the wire may at times recoil on itself and close through, though that seems to revert to the influence of the magnetizing coil in its action.

Professor SILVANUS THOMPSON. Professor Forbes has recalled to my memory what I had forgotten, that quite recently Mr. Hughes made some experiments on concentric tubes. He magnetized them and then tested them, and found the insides of the tubes were reversed.

Professor ANTHONY. I would like to say a word with regard to one

or two of these points. As I understand Professor Thompson, his statement was made with respect to the use of the flat core in place of a round core containing the same quantity of iron, and that in the case of a round core it did not acquire the same amount of magnetization. I should like to know if I am not right in supposing that it was demonstrated long ago that the interior of a round bar of iron was of very little advantage to the strength of the magnet, and we may take out of the interior one-half or two-thirds of the diameter, and retain about the same strength of magnetism in a bar as though the mass were a solid bar. Am I right in that view?

Professor ROWLAND. Yes.

Professor BARKER. Professor Mayer said that the thickness of the shell need be only about one-twelfth of the diameter.

Professor ANTHONY. Now, then, if the mass of iron be flattened down so as to make it very nearly in the center, the result is obtained of putting a large part of the iron in the position of doing the best work.

But what I wanted to speak of in this connection is an experiment that I tried several years ago with a Gramme machine, which was constructed at the Cornell University, illustrating this fact of the self-induction in the machine. We constructed there a machine having a Gramme armature, with alternate coils connected to the commutator on one side of the machine. The other alternate coils were connected to the commutator on the opposite side. So that you understand that the coils connected with one commutator are sandwiched in between the coils connected with the other commutator. Now, in one arrangement of the machine, the brushes connected with this commutator could be so connected with the field magnets that the field magnets were thereby rendered magnetic, the other side of the armature being used at the same time for producing current for the outside circuit of the machine. Now, suppose this to be used for exciting the field magnets, on closing the circuit by means of a wire of very little resistance the machine became short-circuited; in fact, the whole magnetism of the machine would run down in about one-fourth of a minute. I could explain it in another way than that, as these coils passed by the brushes and the current is reversed in them they react so strongly upon the coils connected with the other side of the machine with which they are in close juxtaposition that they completely destroy the current which is in the magnet and run down completely the magnetism of the machine. That self-induction, of course, must occur, as the machine is in use in the ordinary way with all the coils connected up. It amounts to the same thing. Professor Thompson says it is mutual induction and not self-induction. But each one of these coils acts upon its neighbor in the same way as I understand it, when it forms a part of the same circuit.

Professor ROWLAND. I am very glad that that point of hollow magnets has been brought up, as I think that the question of hollow magnets, hollow lightning rods, and a great many similar things, causes more

difficulty, especially to practical men, than almost anything else. It can be explained in a very few words. Take a hollow bar having the magnetizing coil around it acting to send lines of force along it. They have got to go out to make their complete circuit. They could only end at a certain point if we had free magnetism, that is, a separate magnetic fluid. I speak not from a physical sense but from a mathematical point of view. The principal resistance to the propagation of these lines of force is in the air and not in the magnet. If we take away a large portion of the interior of that magnet we will have the surface the same as it was before, and consequently the external resistances are the same. In such a case as that we leave the magnet about as strong as it was before. But that would not be the case if we compress magnetism until we get it up to the point of magnetization of the centre. In that case we should need the whole mass, and it is almost impossible to magnetize to any extent without the centre coming in. It depends on the length of the bar. If we bring the bar around, making a complete magnetic circuit of the thing, so that the lines of force do not have to pass out into the air at all when we put a wire around it so as to wind it like a ring at every point, in that case the whole cross-section becomes equally magnetized, if it is not bent too much. If it is a large ring of small cross-section, it is perfectly magnetized across from side to side. We know that perfectly well; it is a result of the law of conservation of energy. The case of dynamos is like that. We require the whole cross-section to transmit these lines around. The resistance to the magnetization comes partly from this opening and partly from the iron. We have no gain in making these cylinders hollow; indeed we rather increase the outside surface to let lines of force flow into the air. In the case of a dynamo machine, the solid form is not only desirable, but by far the most efficient.

I have thought of that matter a great deal, and experimented upon it. Indeed this closed circuit is the very idea from which the permeability of the iron is determined. All the calculations upon that subject are based upon that law. I think there can be no doubt that in the dynamo the solid form is the proper form, and that the whole cross-section is effective. The whole cross-section of a round piece is just as effective as the whole cross-section of a flat piece. The flat piece exposes more surface to the air, and there is more surface for the force to escape from. That is another reason for not making the magnets flat. The round form is that in which there is the least surface, and therefore the least liability of the lines of force to escape. You can conduct the lines of force by a round piece to any point you desire much better than by a flat piece.

Professor SILVANUS THOMPSON. There are certain objections urged by practical electricians to the use of these very heavy iron cores which come from quite another source. It is that the machine takes such a long time to charge itself with magnetism. It takes a certain

time to magnetize a solid magnet. I have a large electro-magnet in my laboratory, and it takes the magnet ten minutes to get up to its full magnetization, owing to the slowness with which it takes up magnetism. That is held to be an objection by those who want to build self-regulating dynamos, in which the magnetism is required to change instantly. If we want to work partly with shunt and partly with series, however perfectly we balance the number of turns of wire, we do not get what we want unless the iron is divided into masses for the currents to get quickly upon it.

Professor ROWLAND. I do not know that the theory bears upon the solidity of the core. Of course, the more iron in there the better is the efficiency of the machine. I suppose there would be no objection to dividing that cylinder up into a number, so that the Foucault currents could not exist, if the exterior form was round; but I do have an objection to making it any other shape. Indeed, currents could be more thoroughly eliminated by dividing up the cross-section than by making it of a very elongated form.

Professor ELIHU THOMSON. I do not agree that those induced currents can be abolished by dividing up the cores. They can be merely transferred to the wire circuits. The result will be to develop a powerful current in the wire circuit, which will prolong the action beyond the time the solid iron cores would. There is another objection that is made to heavy iron cores. These external currents developed by these cores may set the machine into a constant state of vibration; that is, you have lamps which are adjusted into an arc circuit, and the machine starts on a closed circuit with the carbons in contact. Before the electro-motive force has come up to the proper point the field has become over-saturated or over-magnetized, and the result is that the arcs go beyond where they should go. As the machine goes on in its revolution it cannot sustain that magnetization, because the field is not supported by the same current as at first; the extra current fades away slowly, the carbons go down and shorten the arcs, and a very sudden change of resistance takes place, and the result is a constant trembling of the arc. That difficulty was met in the early development of the Brush machine, and it was overcome by putting a closed circuit of copper around the field. I worked at the same method of overcoming the fluctuation. It can be overcome in another way; that is, by limiting the amount of iron. In an arc lighting machine we can get rid of this bad effect in most cases.

Professor ROWLAND. I do not like to rise so often, but I think there is some misapprehension. I have not said anything about large masses of iron. There are the same masses of iron in my method as in any other. The only question is as to making them round or elongated. Of course by dividing this core up it becomes similar to a core of the Ruhmkorff coil, and the currents change very rapidly. From Professor Silvanus Thompson's remarks, I thought that that was desirable. One cannot say that the current is transferred from the core to the wires out-

side. The same current might take place, and, if the resistances are the same, would take place in the wires outside in both cases. By lengthening the time of action one decreases the electro-motive force or decreases the external current. If the time is ten minutes one would have one eloctro-motive force for the external current; if it is five minutes, the electro-motive force would be somewhere near twice as great as before, the whole quantity of electricity passing being the same in both cases.

Mr. KEITH. In the machine which I have described, and which is a shunt wound machine, there is a constant difference of potential sustained at the terminals, whether there be one lamp in circuit or one hundred. The result is this: The magnetic field is constantly of the same strength; there is scarcely any perceptible variation. The speedy rise and fall of the strength of the magnetic field which is desirable in arc lighting does not prevail in incandescent lighting when the machine is so constituted as to be one of constant electro-motive force. It is desirable in such machines to have a mass of metal which does not suffer speedy magnetic changes of any kind. This has been arrived at as the result of experience as well as from theoretical considerations. I have a machine running for electro-metallurgical purposes, producing a current of a thousand ampères with three volts of electro-motive force. This machine has a large mass of iron in its magnet. The magnets are exceedingly long and made of a pattern which is well known. This machine takes from thirty to forty-five seconds to arrive at say ninety per cent. of its maximum working intensity of field. There is, after that, a gradual rise in its intensity extending over a period of five minutes or thereabouts. This is also a shunt wound machine. If we break the circuit to the field magnets, the current prevails in considerable strength, dying gradually away for about twenty-five seconds from the time of the breaking of the circuit. This construction is desirable for steadiness of current where steadiness of current is desirable; but it is not desirable under conditions where we need to change the electro-motive force, as in arc lighting.

Professor NIPHER. It occurs to me to say a few words in regard to an unfortunate machine which has been referred to several times, and which I saw the other day for the first time. It has two armatures and only two pole pieces. I was as much astonished as anybody could be when I saw it, but when I was informed that it weighed less than one thousand pounds and maintained a high strength of current, I thought it worth while to investigate what the machine would do before we said too much about how it worked.

The resolutions of the Conference with respect to the establishment of a National Bureau of Physical Standards were here read to the Conference as follows, by the chairman of that committee, Professor Newcomb:

Whereas the recent rapid development of the applications of electricity requires the adoption and legalization of common standards of electrical measures, to form the

basis of contracts for the supply of electricity; and whereas, the realization of such standards requires that all instruments for electrical measures be tested and verified by one central authority: Therefore be it

Resolved, That this Conference deems it of national importance that Congress, in pursuance of its constitutional authority to fix a standard of weights and measures, should fix standards of electrical measures, and in order to secure the use of said standards should establish a bureau charged with the duty of examining and verifying instruments for electrical and other physical measurements.

Resolved, That the president of this Conference be requested to communicate the above resolution to Congress through the proper official channels.

Resolved, That the United States Electrical Commission conducting this Conference be requested to appoint a suitable committee to represent the Conference before Congress and to move for such legislation as will secure the object of these resolutions.

S. NEWCOMB.
HENRY A. ROWLAND.
CHARLES K. WEAD.
WILLIAM A. ROGERS.

Captain O. E. MICHAELIS. Mr. Chairman, so long as, according to the President's announcement, the question was confined in the main to the official establishment of electrical units, I listened with interest to the views of the masters on the subject; but now that a wider range has obtained, I crave your patience for a few minutes. To my mind there can be no denial to the necessity of Government adopting standard units for the transaction of its own business. The Constitution grants to Congress the power to "fix a standard of weights and measures"; and laws have been enacted in the exercise of this prerogative, all, however, with a single exception, applicable only to Government officials. Thus we have the standard troy pound, in the custody of the mint in this city, upon which our coinage is based. The Commissioner of Internal Revenue is authorized to issue hydrometers and saccharometers and weighing instruments for the determination of revenue questions; and the metric system has been legalized, and by a special enactment, with the exception alluded to above, no contracts can be voided because its details are expressed in metric measures. You will observe how carefully Congress has abstained from forcing standards upon the public at large. Their practical regulation has been left to State and municipal authorities, and I take it that this is true Republican doctrine. But Government can act by example, and, from the nature of the case, its example must be followed by all. I assume that we are all agreed upon the necessity of having Government standards, and the question really is to obtain them and to cause Congress to adopt them.

How can this be best done? I have had some little experience with Congressional committees, and they are most influenced by concrete arguments; by showing them that the thing asked for is necessary for the welfare of their constituents or for the material good of the Government. Sentimental arguments rarely produce tangible results. I belong to a department of Government that is vitally interested in standards of length. At Frankford Arsenal, one of our large manufact-

uring establishments, we have to day virtually four working length standards—the Coast Survey, Brown & Sharpe, Stubbs, and Pratt & Whitney. So much practical difficulty was experienced on account of variations in scales, that some years ago we were compelled in ordering work to furnish maximum and minimum gauges and templets for the benefit of the contractors; and in making gauges for issue we now decline to give their dimensions, but call them maximum and minimum gauges for the inspection of the special product to which they may apply.

It would not be farfetched to imagine a gun-tube fabricated in Boston and machined to an exterior diameter of 20 inches; shrinkage rings might be ordered from Cleveland, to be finished to a diameter (interior) of 19.998; the Boston contractor might use a Brown & Sharpe; the Cleveland a Stubbs gauge, and the inspector might apply a Coast Survey or Pratt & Whitney standard and reject both tube and rings. Of course this is improbable, yet it is possible. And in smaller work similar *contretemps* have occurred within my experience. Examples like these show the necessity of Government having standards; and once established, no difficulty would be found in inducing Congress to enact that they shall apply to all Government work.

Indeed I believe that this may be done by an Executive Order without special legislation. What is the most practical way of obtaining them? The creation of a new Bureau I do not deem feasible at present. Various Departments of the Government are now charged with portions of the work, and we all know how loth the head of a Department is to transmit it to his successors shorn of some of its functions. Hence the establishment of such a Bureau might at the outset meet with quiet and powerful opposition, whose weight all who have been in Washington must appreciate. Still there is a simple plan which would bring about desiderata so forcibly presented in Professor Snyder's paper, without friction, without opposition—one which would enlist the active support of all officials. Under the Treasury Department we have the Light-House Board, the Coast and Geodetic Survey, and the Mint. Under the War Department, the Engineer and Ordinance Corps and the Signal Service. Under the Navy Department the Bureau of Inspection of Ordinance and the Naval Observatory. Under the Interior Department the Patent Office; all vitally interested in this matter of standards of every kind and description. Let representatives of these various branches of the public service, aided by those distinguished scientists who have made this matter a study and the practical men who have in reality given us our standards, be convened for the purpose of establishing Government standards, and I believe the result desired will be achieved.

Thereafter with the growth of the subject there might be hope of the creation of a special bureau. The use of standards should be made

mandatory in all official transactions. Their universal adoption would, without the compulsion of law, necessarily follow. For it is very evident that no one could keep up the expensive plant required by the use of varying standards. And as Government is the heaviest user, its requirements must evidently govern.

The practical execution should be left, as is everything else virtually of moment under our system of Government, to private enterprise. The admirable work done by Brown & Sharp, and Pratt & Whitney for length standards tells us unmistakably what would be done in other directions were the proper opportunity offered.

Professor NEWCOMB. It strikes me that there is a slight misapprehension as to the manner in which this thing is expected to be brought up, namely, that we have got to present cut and dried plans in all their details, which are to go through Congress exactly as we plan them without any change whatever. We do not attempt anything of that kind; we simply let Congress know in a general way what we want; and next winter this committee will be before Congress to discuss the manner in which this end can be obtained. Perhaps it will be necessary to have a new Bureau; perhaps some existing Department will take charge of the matter; perhaps it may be incorporated with some new department. All we want now is to have the matter brought before Congress in an authoritative way.

Professor ROWLAND. I think there is nothing in the resolution which indicates the means by which these objects are to be obtained. The means are not under discussion; this is only the expression of the scientific men of the country that they wish to have some means for verification of their electric units.

Captain MICHAELIS. As I understand it, the resolutions commit this Conference to the establishment of a Bureau for the establishment of and verification of physical standards. If I am wrong, I beg to be corrected.

Professor NEWCOMB. The resolutions do suggest a Bureau. I will suggest that if Captain Michaelis will put his ideas in a more condensed form I will put them as a substitute to this motion. In place of "Bureau," say "authority," if Captain Michaelis will move that as an amendment.

Mr. PREECE. The idea of a Bureau is the milk and substance of the resolution; and the most powerful arguments introduced and read in favor of that motion were those just read by Captain Michaelis himself. It appears to me that these resolutions of Professor Newcomb meet all our views. [Applause.]

On motion, the resolutions were adopted by the Conference.

Adjourned to 10 a. m. Friday.

FIFTH DAY.

SEPTEMBER 12, 1884.

The Conference met at 10 a. m.

The PRESIDENT. Before commencing this discussion I would like to say a few words. We saw yesterday the relative value of theoretical and applied science as applied to the subject of the dynamo machine. We did not find that theory was of no value except as it agreed with applied science, but we did find that the two ought to agree with each other. In none of the applications which we have recently had of electricity have any new laws in regard to electricity been discovered. If a person had a perfect brain, he should be able to evolve a dynamo-electric machine, or any other machine of which the principles are known, out of his own mind before he had tried a single experiment. Every law of electricity necessary to be known is already known; it is only a question of the brain that has the power to evolve the perfect machine, and when we say that theory does not agree with practice, it means that we have not got brains enough to apply the theory to the facts and get at the result. It is impossible for a man who is finite in all his faculties to apply the theory perfectly; the machine becomes more and more complicated; he has to consider too many points. In considering one point he loses sight of another point; his mind is not large enough to grasp the whole machine and balance these various conflicting, I will not say forces, but various conflicting ideas. That is the reason why, after making a machine, we have to try it to see whether it is going to work. But the ideal is to evolve from one's mind a perfect machine, taking account of everything, set it to work, and it goes.

Professor E. J. HOUSTON. This seems to be a question as to difference between theory and practice, and I differ from the president in his remarks. I doubt whether the statement that he made will be taken as substantiated, that we know all about the laws of electricity. I doubt, indeed, whether it will be possible to ascertain the laws of electricity except by the method of experiment. In my mind it would be impossible to divorce practice and theory, or theory and practice, whichever you put first. Theory comes as the result of practice, and all that precedes that is hypothesis. We may speak, then, of the hypothetical man, but we ought not to speak of the theoretical man until the theory and practice thoroughly agree. A great deal that passes as theory ought strictly to be ranked as hypothesis.

The PRESIDENT. This subject is not upon the programme, and I did not mean, in stating my views, to raise a storm of this kind. Perhaps I had no right to bring it up at all, and furthermore no right to permit a discussion upon it. By experiment we find out the true theory. Of course the whole thing is based upon experiment—that is understood. If this is a question the Conference would like to discuss, it can be put upon the programme, perhaps, for to-morrow.

The first subject upon the programme is "The Electrical Transmission of Energy." I call upon Professor Nipher to open the discussion.

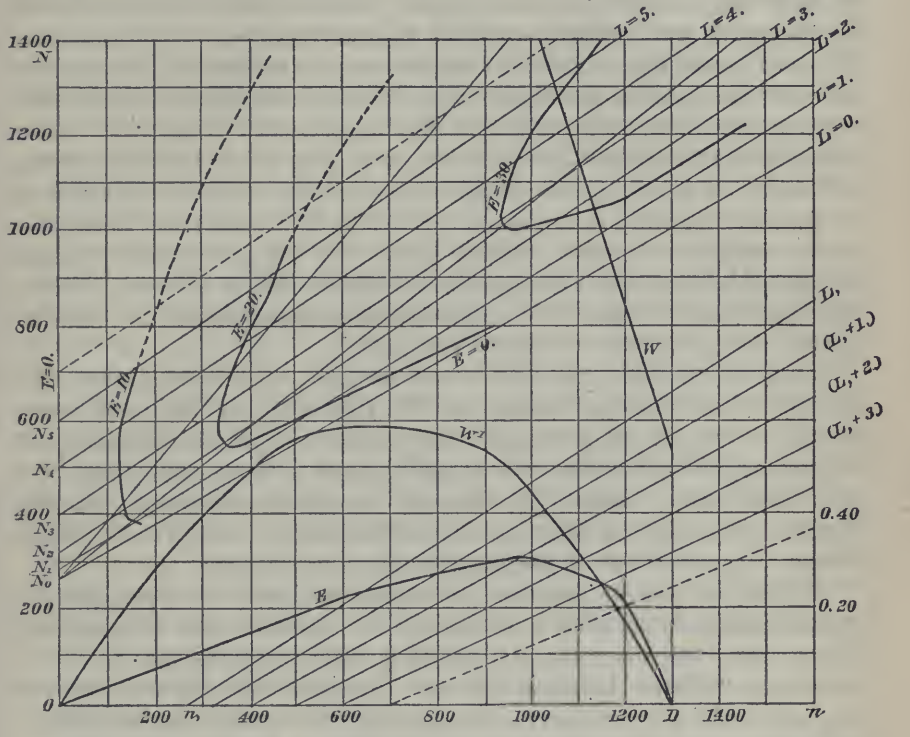
Professor F. E. NIPHER. Some time last year I began an experimental investigation on an electrical generator and a motor with the view of determining the relations between the quantities that were involved. Perhaps I could bring out my ideas more clearly if I were to put them in the form of a graphical solution of the problem.

Suppose we have two machines, A running as a generator and B as a motor. Let N represent the number of revolutions per minute of A's armature, and n the number of revolutions of B's armature. If we run B as a motor without any load on its pulley, the relation between the speed of the two machines will be represented by a straight line, this [indicating a height N_0 upon the vertical axis] being the speed of the generator when the motor is just about to start. If we put a load of one pound upon the motor, the generator will be brought to a still higher speed (N_1) before the motor starts. The relation between the two speeds will be again represented by a straight line. The greater the load upon the motor the further up these lines will be drawn. I will indicate the load upon the motor by L. This load, which is constant, being determined, suppose that the two machines are connected with dynamometers of the form suggested by Professor Brackett in such a way that the power applied to either of the machines may be determined when it runs as a generator; and let the power yielded by the motor be measured, let us say, by a Prony brake. These enable us to determine the amount of power which is applied to the generator and the amount of work which is delivered by the motor. Of course, by making the corresponding electrical measurements the friction, and so forth, would also be determined; but I shall speak mainly of the total work which is applied to the generator, and which is delivered by the motor, brush friction and all that kind of thing being counted in.

We shall have three surfaces determined by these observations, viz, the surface of work done by the motor, the surface of work given to the generator, and the efficiency surface; that is to say, at each point on these lines we shall be able to construct the ordinate of a point in the surface of useful work W^1 delivered by the motor, the ordinate of a point in the surface of total work W applied to the generator, and the ordinate of the efficiency surface E, or the ratio W^1 W. The section of these three surfaces, making N constant, is the so-called Kapp section.

We shall have in the section a gradual increase in the work delivered, which will at some speed or other of the motor reach a maximum. The total amount of work applied to the generator will be represented by nearly a straight line—so nearly so, with the machines that I have worked with, that it could not be considered anything else. The efficiency surface will be represented in the section by a curve running to a maximum like this (O E D), and going down to zero at this point (D).

The lines of equal efficiency are more interesting than the lines of useful work delivered. They will be represented on this plane as contour lines. This is the first line (E=0,) and you see how it is zero here (along O N) because the motor does not revolve, and zero here (along N_0 L) because it carries no load.



These curves, with the exceptions of the lines (n_1—L), &c., which are only inserted at random for illustration, were experimentally plotted by Professor Nipher. The motor was a light Fuller machine, having the armature and field connected in series. The resistance of the generator was about 50 per cent. greater than that of the motor. The resistance of the line connecting the two machines was one-tenth the combined resistance of the two machines. The curves marked E, W, W_1 form the so-called Kapp section, having been first published by Gisbert Kapp. They are respectively sections of the efficiency surface, and the surfaces of total and useful work, by the plane N=1,000 (revolutions per minute.) For further information with regard to these experiments see Journal of the Association of Engineering Societies, November, 1884. Professor Nipher has lately succeeded in perfecting the methods of measuring power in such small quantities, and is continuing these experiments.

We would have a set of surfaces similar to these three on the other side, and which would be symmetrically placed if the two machines were alike. These lines of constant load would be parallel with like machines and would always diverge if the machines were not alike. The distance between these two lines 4=0—L_1=0 would be increased by putting in a line resistance. If the two machines could be made reversi-

ble, these lines would coincide, and the two sets of surfaces would touch each other along that line.

Suppose that we have reached this point where the generator is running a motor without a load. If we put a belt on the motor and cause it to run at a higher speed, then, of course, it will require less power to drive the generator, and at a certain point the back current produced by the motor will exactly neutralize the current given by the generator. In other words, the current will become zero at that point. If, then, we increase the speed of the motor to a still higher degree, we shall pass into the corresponding surface on the other side. Here (n_1) is where the machine B, driven as a generator, will just drive the other when unloaded. The pull on this machine is a minus quantity, because it is in opposite direction to the force applied on the brake-arm. Now, at a point midway (of course, whether midway or not would depend upon the two machines) the work applied is simply that of friction. To the efficiency curve we can give no physical interpretation in this case, because the signs of the two works are unlike.

When we pass into the other set of surfaces, the other sheet of this surface W^1 becomes the surface for W; that is to say, the total power applied. We are going through this surface, which may be called the negative surface, in a direction at right angles to the Kapp section. In this direction the surface of efficiency has also a maximum, but the surface of useful work delivered has no maximum; that is to say, these lines run upward, but do not pass through a maximum.

My purpose was to examine and determine, first, the constants for these surfaces as accurately as possible for certain types of machines, taking some one machine as a standard and comparing others of the same type with it. I should then get empirical formulæ which would be sufficiently comprehensive to represent the whole case, and which would be more manageable, perhaps, than those which have been theoretically deduced by Clausius, which are really approximations. It would also be advisable to experiment on different armature-types of machines—for instance, the Gramme, which is the one I have already used, and perhaps the Edison machine. I simply present these views of the work in order to find out what may be thought of it by the members of the Conference. It seems to me worth doing, but I would like to hear some other opinions on the subject.

Mr. SPRAGUE. I would like to ask as to the Gramme machines, how they were excited, whether the field magnets were in series with the armature, or whether they were compound wound.

Professor NIPHER. They were compound. I think in compound-wound machines we found some of those lines to vary very differently from those of the series wound machines.

Mr. PREECE. I would like to refer to a matter touching upon the electrical transmission of energy which may interest the members of the Conference, and that is, the transmission of currents of electricity

for the purpose of producing artificial illumination. You have in practical use in large cities in this country various types of machines for electrical lighting, and large plants where the electrical energy produced at one end of the line is sent out to various points along the line for the purpose of converting it into luminous energy. Some of the lines I know from experience amount to as much as twenty-one miles of the copper wire used in systems of arc lighting. It, therefore, becomes a matter of no little interest in considering the efficiency of the dynamo-electrical machines to consider their efficiency, not so much as regards their ability to convert mechanical work into electrical energy, but, also, in another sense, of the ability of the electrical energy so produced to be readily transmitted into radiant energy. This is a matter of exceedingly great and practical importance.

The question to my mind is not as to what is the best type of machine for producing the highest possible equivalent of electrical returns, but as to what is the best type of machine for producing that kind of electrical energy which can be transferred to a distance, and which can be most economically converted into light. This, of course, would necessitate the consideration of whether, according to the knowledge we have, we can progress much further and utilize incandescent or arc lighting. We will take up first arc lighting (I am merely suggesting this as a matter of thought to the Conference); and this idea would appear to come up, whether it is not absolutely necessary, in taking into account the economy of the dynamo-electrical machine, to consider the number of foot-pounds of energy absorbed by the machine and returned, not in electrical energy, but in candle-power at the lighting point. Suppose a given amount of energy was present in the electrical current, the question would be which would give the most economical return of light, a high electromotive force or a low electromotive force.

Professor NIPHER. I observe that I left out one thing which I intended to bring in, and that is, that the lines of equal efficiency are represented by lines which rapidly increase in distance from each other—that is, the distance between successive lines of equal efficiency become farther and farther apart. The line giving the maximum work which passes through the vertices of the efficiency curves is also a straight line or very nearly straight.

Professor FORBES. I think it is a pity that a discussion on Professor Nipher's paper should cease immediately, although it seems almost necessary that it should be so. It seems impossible in the brief way in which he has brought it before us to grasp the full bearing of the experiments made and the conclusions to which the experiments have led him. But it would seem that such an intensely suggestive method as he has brought before us should have more attention than we are able to pay to it from the brief description which he has given us and also from the fact that the subject is a new one.

I think it very desirable that Professor Nipher should let us have

the benefit of the results of his experiments, and the conclusions, in order that we may be able to incorporate them into our work, if it is to be carried on in future.

Professor NIPHER. I would like to say that it is my intention to carry on this work, and I presume I shall put the greater portion of next year upon it, provided it is not said that it has already been done or that it is hardly worth while to do it. I do not know but that a remark of that kind would discourage me a little.

Mr. SPRAGUE. I would like to make one comment on what Mr. Nipher has said. It might go to the question of efficiency. We have to consider not only that we can get only a certain percentage out of, the dynamo itself, but also the fact that its energy must be distributed over long circuits. Take an underground or overground system, which may be readily carried over a square mile of territory with a loss not exceeding ten per cent. with recently constructed dynamos. The electro-motive force of the generators must be very high; otherwise we are led to the fact that we are obliged to use underground conductors which must be of such large size that our space over which the energy may be transmitted for lighting would be altogether too large to enter into any commercial consideration at all. The same for arc-light circuits. We must therefore deal with very high potentials. Because we use high potentials we can use much longer lines and smaller conductors. Mr. Preece speaks simply of the use of arc-light circuits for the purpose of illumination. I think just as wide a purpose would be that of transference of power, and those motors which can be perfectly self-regulating can be brought into circuits with arc lights and run just as efficiently as arc lights are to-day. They will be just as self-regulating and controllable as the arc lights are.

I want to place myself on record as to what I think the future of the electric railway is going to be in this country, and that is, that high potential circuits are going to be used. The idea has been brought forward of low potential circuits, as low as thirty or forty volts. Also, the suggestion is to use one of the rails for conducting the return circuit. These questions will have to pass through the same stages as in electric lighting. We started out with one conductor and used the gas-pipes as a return. We are using now closed metallic circuits for all kinds of lighting, whether arc or incandescent; and precisely the same thing will be done in the electric railway. The rails will not enter on any great line as one part of a circuit; there will be a closed metallic circuit and a potential of four or five hundred or a thousand volts, and sometimes as high as two thousand volts.

I will give an instance in which low potentials are used and the disadvantages of such. The maximum mechanical effect is when the counter electro-motive force is one-half the initial. Let us take an electro-motive force of fifty volts; I suppose that is high enough. The greatest mechanical effect is when the counter electro-motive force is 25 volts. We

average 800 watts per horse-power, when it comes to practice. If we want to transfer ten horse-power, we have 8,000 watts to take care of. The electrical energy is the product of the current by the electro-motive force. Our current will be 320 amperes. In other words, in order to get that initial electro-motive force of our motor of 50 volts, when our motive power has the greatest mechanical efficiency, we have to use 16,000 watts to transmit 10 horse-power. Suppose we transmit that over 500,000 circular mills, that is a very fair-sized conductor. Suppose we have a single mile over which to transfer from our motor through our conductor and back again. In other words, we have something over 10,000 feet. The resistance is not less than .23 of an ohm to and return from our generator. The resistance of the generator with an electro-motive force of 25 volts and 320 amperes of current would be about .073 of an ohm. We have a resistance in our conductors which is three times that in our generator, with the generator working at its best mechanical effects. We have not less than one-sixth of our energies supplied to the generator used up in the motor itself. I think it is not necessary to go into the question of longer distances when we are dealing with 50 or 100 or 200 horse-power at low potentials. We must use 500, 1,000, or 2,000 volts, it is perfectly certain, and the rails must disappear from parts of the circuit, and many safety devices be introduced to take care of the high potentials.

The PRESIDENT. I call upon Mr. Preece, of the Postal Telegraph of England, to say something in reference to storage batteries.

Mr. PREECE. I have been called upon somewhat unexpectedly to open a discussion on this question of storage batteries, and I regret very much that I have no notes with me to refer to, and that I shall have to trust a good deal to my memory. As I am not possessed of a poor memory, I do not think I shall develop any very serious error, although Mr. Edison himself has declared that this question of storage batteries has the most remarkable power of developing man's latent capacity for lying. [Laughter.]

I very much agree with Mr. Edison's definition, for I think there has been more lying, more swindling, and more rascality over this question of storage batteries than over any other department of electrical science. These batteries have been very improperly called storage batteries; I very much prefer to call them secondary batteries. This subject is one upon which there is a good deal of misconception, which has arisen from the introduction of the word "storage."

These secondary batteries have been before the world now for over twenty years, and all the physicists who go to Paris almost invariably meet with Planté himself, the father of this instrument. For the past twenty years, nearly, it has been one of my great pleasures when I visited Paris to pay a visit to M. Planté, who has shown me, as he always shows everybody who sees him, the progress that he has made. Until M. Faure conceived the notion of coating the plates of lead with the red

oxide of lead to assist their preparation these secondary batteries had not attracted much attention, although Planté's papers deserve the most careful consideration from all electricians. M. Faure found that by coating the positive plate of the secondary cell with a layer of minium, which is the red oxide of lead, he considerably hastened the preparation of the plate. Planté himself simply depended upon the electrolytic action of the current in peroxidizing his plate. This was an operation that sometimes involved months. M. Faure did this in as many hours. Unfortunately, Faure's invention fell into the hands of a man who put forward one of the most diabolical schemes that Paris has ever produced. A man by the name of Phillippart placarded all Paris with the most outrageous notices of what this great battery was going to do. Batteries were to be distributed, like milk and ice, at our doors, so that motive power and light could be obtained and used in our houses. The result was that a considerable sensation was created, and an attempt was made to bring out a financial swindle, but it did not succeed. Another gentleman was associated with him by the name of Volckmar. A Mr. Sellon joined himself with Mr. Volckmar, and a company was formed in London to develop this scheme, and a great deal of money was collected, but most, if not all, of it has been lost. The fate of Mr. Volckmar was sad indeed, for about a month ago his body was found in the Seine with a bullet-hole through his forehead.

Now, two or three other men had been working scientifically at this idea, particularly Professors Tribe and Gladstone. They approached the matter in a truly scientific spirit; investigated it most carefully, and have determined the chemical actions which take place, and developed the laws. The result has been that Mr. Tribe himself has brought out a form of secondary cell, although it is not yet in the market, that is a great improvement upon anything that has ever been brought out hitherto. Many other persons have been working silently upon this subject, and have gone back to Planté's original work and have followed Planté's original method. The batteries that I am going to speak about are merely modifications of Planté's simple lead cells, the simple cell being two plates of lead placed in dilute sulphuric acid, one plate oxidized and the other plate pure.

As regards the use of secondary batteries, I have made a great many experiments with them, and, first, I will speak of their use in telegraphy. Some time ago the idea was mooted that a considerable economy might be effected in the working of telegraphs if dynamos were used for that purpose. Dynamo machines are used by the Western Union Company to work their circuits centering in New York. We tried them in England, but they failed with us, and for a very obvious reason, which is simply this, that in England we do not work with "closed circuits." We work our instruments with rapid reversals. Our automatic system, which is very extensively employed, is worked sometimes at the rate of 300 words per minute, with very rapid reversals of very

minute currents, and it is absolutely essential that these currents should be uniform in their character. The currents produced by the dynamo machines are not uniform. If a telephone be inserted in a secondary circuit of an induction coil of which a dynamo machine forms the primary current, I have invariably heard sounds with the very best dynamos I have tried, which are indicative of variations of the current fatal to a fast speed of working; but when we utilize the current from a dynamo in preparing a secondary battery, we then have a means by which we can produce currents of absolute uniformity. The secondary battery has the great advantage of giving a battery with a very low internal resistance and a very high electro-motive force. The electro-motive force of an ordinary secondary cell of Planté's form may be taken as a rule to be two volts; its internal resistance may be made whatever you like.

I had a series of secondary cells made, one set being a Faure-Sellon-Volckmar, and another Dr. Tribe's, and the third set being simply Planté's. There were eight cells to each set. From each of these sets forty circuits were worked. All worked for about three months (I think the exact time was 96 days) uniformly and perfectly, with no trouble that would necessitate even the glancing at them during this period. Then suddenly one failed, and immediately afterward another failed, and then the third failed. They were charged up again, and then they went on for another three months (I did not repeat the experiment on the Faure-Sellon-Volckmar one) and they have behaved as well as one could possibly wish; so much so that one of the first duties that I wish to discharge on my return to England is to arrange for their use on a large scale at our general post-office. We have at the general post-office in London six hundred and fifty circuits centralizing there, and are now using about twenty-two thousand cells. I think it more than likely that we shall be able to work the whole of those circuits with probably not more than five thousand secondary cells.

As regards electric lighting, I have already used secondary batteries for that purpose in the post-office and my own house. My house is in a portion of the country through which no wires pass for the purpose of electric lighting within a reasonable distance, and I am dependent on my own supply. I light my house by gas, but I burn my gas in my garden, and I extract from it that which I want, namely, light; and I discharge into the air of my garden that which I do not want, namely, poison. My gas is employed in working a small gas engine of 2 horse-power. The gas engine works a small Gramme dynamo, which, when worked at its full power, gives me 42 volts and 52 amperes. These 52 amperes are directed into seventeen cells, the cells being Planté's original cells. The plates themselves are 2 feet square, and in each cell there are twelve of these lead plates. The lead plates are made up of four thin sheets of lead, each sheet being about one thirty-second of an inch thick, and perforated in squares regularly all over to facilitate diffusion. These four thin plates are tied together—almost woven to-

gether, with thick worsted. They are arranged in pairs, six on one side forming one pole, and six on the other side forming the other pole. They are placed inside of an ordinary pitch-pine box well pitched inside, and the insulation of the cell is maintained by a thin India rubber bag, which lines it. When the solution is put in, it forces the India rubber out, and the bag takes the exact form of the cell, making a cell thoroughly water-tight, and thoroughly electricity-tight. I have seventeen of these cells all arranged in series. My gardener has the gas-engine under his charge; he is an ordinarily intelligent gardener. When he comes on duty in the morning he lights the gas in the gas engine and starts the dynamo which charges my battery, and when he goes away to his dinner, after the engine has been working for three or four hours, the battery is prepared. I go home in the evening. I have got a store of electricity. I have every room in my house fitted up. I have at my bedside a most charming mellow light, with no smoke, no wick, no heat, by merely turning a tap. I have the softest and most delicious light you can conceive of, thrown upon my paper or upon my book. I have had this going on for four months, and I have never had any bother, except on one occasion, when the gardener put his foot on the wrong place and the head of the gas-engine came down upon it, crushing his toes, and the poor fellow has had to lose four of his toes. Their removal reminds him of the dangers of electric lighting. [Laughter.] It would be impossible to overestimate the importance of this use of these storage batteries. I have a strong impression that for all isolated places similar to mine the storage battery is an essential thing, and I think that some one is bound to work it out. Sir William Thomson spent much time in investigating this subject, but has not succeeded, simply because he confined his attention to the Faure-Sellon-Volckmar form of battery. I confined my attention to the plain, simple Planté cell, untrammeled by any patent. Anybody else who chooses to follow my example can do precisely the same thing.

There are one or two other uses for secondary batteries. I have been very luxurious and extravagant in carrying out this arrangement; for instance, I have a little daughter who has a very pretty doll-house. Her doll-house consists of four rooms, and each room is well furnished and well populated with dolls and supplied with a charming little electric light. Then I have a cigarette-lighter which consists of a spiral platinum wire which can be rendered red hot by the current. Supposing your house fitted up with 55-volt lamps; you do not wish to send that whole current through a doll's house or a cigarette-lighter. But putting into the circuit a secondary cell by means of a switch diminishes the light by setting up a counter electro-motive force of two volts for the time being; and when I connect the poles of that secondary cell with a platinum wire, a current passes through, due to the two volts from the secondary cell, which heats the platinum wire quite sufficiently for the purpose of a cigarette-lighter, and I extract a current from the main

circuit by means of the secondary cell to light up the doll-house. I have also a microscope fitted with little lamps, which are supplied with current in the same way.

There is another field in which satisfactory experiments have been made with secondary cells, and that is in lighting up trains of cars by electricity. The railway company between London and Brighton has been for some months past lighting up one of its express trains by electricity. The dynamo is worked by the motion of the wheel, and charges up a secondary battery during the time that the train is working. The current is being extracted from the battery at the same time, giving a light in the carriages which is perfectly steady and quite independent of the motion of the train.

That leads me to a point which I omitted, and which led me to work so hard at secondary batteries, and that is that a secondary battery fixed as a shunt on the main circuit renders the current produced by the inconstant engine and the inconstant dynamo perfectly steady. If your engine is unsteady, its pulsations will increase or decrease the light in the incandescent lamp, producing a flickering. This has been a fatal objection to the use of gas-engines.

It is a difficult thing to say that in making an appliance of any kind we have reached absolute perfection; but I can say this: That a proper secondary battery inserted in the electric light circuit renders the current uniform, and the light is as near perfection as we could wish. Suppose that a lamp requires fifty volts; then we will require twenty-five secondary cells. The first current that goes through simply charges up the lead plates, and we get a counter electro-motive force from that battery of fifty volts. No current whatever passes through the secondary cell after it has been raised up to fifty volts; and the whole current passes through the lamps unless there is a variation of the engine. Then the energy that is stored up in the battery makes up the deficiency, and in that way we get the steadying effect of the secondary battery.

There are certain defects that have developed themselves in these batteries that have been gradually cured. The great defect in all secondary batteries is buckling, an effect due to the formation of peroxide upon the plate. A lead plate when it is peroxidized changes its dimensions, and unless some precaution is taken it will buckle up into all kinds of positions and the two plates may come into contact. This difficulty is overcome by separating the two plates by means of paraffined wood. The most effective material that I have tried is ebonite. Ebonite grates are easily made, are very light and thin, and quite prevent all buckling.

Secondary cells have not been in use long enough to enable us to determine how long they will last. I have had them in use for four months with, in one or two instances, signs of a little disintegration of the positive plate. I think that the plates will have to be renewed only about once in every two years. But I do not care whether I have to renew as

often as once in every four months. The charm of the whole thing is such, and its cost is so trifling, that I will keep it in my house, and I am quite sure that all those who have worked in the same direction will never give it up. I have protested often and often against the comparison that is drawn between the cost of gas and the cost of electricity. The two things are not to be compared. When we indulge in luxuries we do not compare the cost of the luxuries with other things. If you want fine 1834 port, you do not compare its price with common ordinary claret. If you want to indulge in a fine pheasant, you don't compare it with the old cock that crowed before Peter. So when you have a delightful luxury like the electric light you don't want to compare its cost with that of deleterious gas or dull sperm candles, or any other mode of illumination by which life is shortened.

When enterprising light companies will bring electricity to our doors we will all take it, so that I look upon the days of gas as an illuminant as being numbered; but gas, as a generator of power and heat, is just in its infancy.

There are many facts connected with the working of secondary batteries that I should have liked to have given you. I should have liked to have given you the efficiency or the ratio between the work put in and that put out. I have made careful experiments as to the efficiency of storage. Up to the present time I have not succeeded in getting more than 40 per cent. of the power put in. I do not care if I get only 20 per cent.; I get what I want, and that is all I care for, whatever the cost may be, so long as it is within my means.

The cost is certainly within the reach of every man in this room. Every man in this room can afford the comfort which this light will give him, and an electric light will enable him to live four or five years longer in this world than he would without it. [Applause.]

Professor CARHART. I wish also to add my testimony to a point which Mr. Preece has mentioned, and that is to the courtesy uniformly shown by M. Planté to any one who visits him at his laboratory and who is interested in his secondary battery.

It is also a point of interest to know, as probably some of you do not know, that Planté himself tried the method many years ago of covering the plates with minium. If Professor Barker were here, he would bear witness to the fact that Planté said to us both that he tried the same thing several years ago, and found that in the end it was not so effective as the method which he adopts, which, though necessarily slow, results in a better form of cell. It is gratifying to learn from Mr. Preece that we are advancing, so far as new experiments are concerned, and that we are finally going back to the original form devised by Planté.

We are all well aware of the careful and interesting experiments referred to by Mr. Tribe on the chemistry of the secondary battery. These experiments seem to show that there is a greater or less forma-

tion of lead sulphate in the action of the cell. That also shows us why the electro-motive force, after having run down, is recovered to a certain extent after a period of rest. A great many points have been cleared up that were certainly quite enigmatical before. Some experiments, I think by Professor Barker, go to show that in the Planté cell, as formed by Planté, by a rather slow and tedious process, there is only, to say the most, a small amount of lead sulphate produced. I take it that the giving out of the cell is due to the formation of lead sulphate, so that if these cells are formed by the rules laid down by Planté they will have a tolerably long life.

Just one word with regard to gas. You will remember that a year or so ago Professor Langley showed, I think beyond any question, that the percentage of the radiant energy from an Argand gas-burner, that is effective in producing illumination, is less than 1 per cent. It certainly is a very curious result. If this is so we can well afford to recover only 40 per cent. of the energy put into the secondary cell, use incandescent lamps by the aid of secondary cells, and still be within the economy of gas.

Mr. N. S. Keith. In 1878, during experiments I was then carrying on in the electrolysis of lead, and in collateral experiments which relate to the chemical action of lead in various solvents and in various electrolytes, I was led to make a secondary battery, after having made one of Planté's, by coating the plates with peroxide of lead by electro-deposition from a solution of lead in which the plates were immersed. I took 240 cast pieces of lead plate, of one foot square each; I divided them into ten cells, making 24 plates in each cell, 12 of which were positive and 12 negative. By using a suitable solution of lead, a subacetate of lead and nitrate of sodium, and by treating that with a current of electricity, I produced decomposition of the solution, deposition of peroxide on the positive plate, and at the same time metallic lead was deposited upon the negative plate in a very finely-divided and crystalline state. Some hydrogen was also deposited with the lead, an equivalent to the extra oxygen that was deposited in chemical combination with the lead upon the positive plate. These plates, after pressing the metallic lead so as to cause it to cohere and adhere to the plate, were taken from this solution and immersed in sulphuric acid, and gave a very satisfactory secondary battery indeed. At that time, in May, 1878, I ran a dynamo as an electric motor with this battery, and I effected some chemical and electrolytic decompositions in a solution of sulphate of copper, and did some other things which were then the only possible commercial applications of secondary batteries. Since that time electric lighting by incandescence has come to the front, and we find the great use of secondary batteries which has been so forcibly set forth by Mr. Preece. At that time we did not have telegraphy by the dynamo; but we had telegraphy by the use of the primary batteries with which

we are all so familiar. I have since that time carried on a considerable number of experiments relating to secondary batteries.

The whole subject is an electro-chemical one. There is a chemical decomposition of the solution by the passage of a current of electricity. There is a decomposition of the water of the electrolyte either primarily or secondarily. Oxygen goes to one plate, hydrogen to the other. For a new experiment I have taken solutions which suffer decomposition, without delivering free hydrogen and oxygen. I have taken, for instance, solutions of the subsalts of lead. The current causes decomposition, and a deposition upon one plate of peroxide of lead, and upon the other metallic lead. For instance, I have taken for one plate a plate of carbon and for the other a plate of lead. A solution of a low subsalt of lead was used as an electrolyte by passing a current of electricity through it. The action is to raise the subsalt to a protosalt. This takes energy, which is absorbed during and through the chemical action. When the source of electricity is removed and the circuit remade, the current of electricity is returned until the peroxide of lead and the lead have again entered the solution. The action of the charging current of electricity is to raise the electrolyte to an acid condition. But the charging action is stopped before that point is reached because the end would be partly defeated in passing it.

I am also carrying on some other experiments in the use of secondary batteries. The idea is to preserve the integrity of the oxide plate for an indefinite length of time, say as long as may be practically worth while. So far, after some months' use, I have been able to oxidize the plate to the desired depth, say one-sixteenth of an inch, and preserve a backing of metallic lead. Under this change, if my theory and practice be correct, I will not be obliged to replace, as in the present known storage batteries, the oxide plate.

Mr. KOYL. I should like to say something in reference to the theory of the secondary battery. For a long time we have had secondary batteries of some sort or other. It is to Planté we owe our knowledge that there are no two substances which, placed in juxtaposition in a secondary battery, will give the electromotive force that peroxide of lead and spongy lead of the Planté cell will do. It remains the same as it was in 1859, when Planté experimented and wrote.

It therefore follows that a battery with these two substances will exceed batteries with other substances in efficiency, because a battery of high electromotive force is better than one of low electromotive force. Batteries of low electromotive force are not exactly useless, but the next thing to it. Peroxide of lead and spongy metallic lead we may take as two substances which should be used in secondary batteries. The simple case is to use masses of these substances, with wires down in them; but we are met instantly by the fact that the resistance is high, so that we must use some form of plate so that the resistance may be low.

Our battery must be a box of some sort, with two plates in it, the one coated with oxide of lead and the other with spongy lead, having their surfaces in contact with the liquid containing sulphuric acid. The necessities of the plate, then, are the only difficulties to be considered. They are three, and they are very simple ones. The plate, first, must be a conductor of electricity, and that leaves us the metals and carbon; no others, I believe. The plate, in the second place, must resist the ordinary chemical action of sulphuric acid. That leaves us lead, four or five expensive metals (platinum, gold, and so on), and carbon. We have practically for this purpose only lead and carbon, the expense of the other metals being too great to allow them to be used extensively for commercial purposes. Our third and last condition is, that the plate must resist both the electrolytic action and the oxidizing action of the current. Lead will resist the electrolytic action of the current moderately well; but the oxidizing action reduces the lead to an oxide in a very short time. That leaves us only carbon for the secondary battery of the future. I think there is no way of controverting that argument. I should be very glad to know if there is.

Now, then, to the manufacture of the carbon plate. The capacity of the storage battery depends entirely upon the amount of lead and peroxide of lead which you can put upon these plates. They are the substances in which the chemical action takes place, and the quantity which you can put on the plates determines the capacity of your secondary battery.

It is possible to manufacture carbon plates by the ordinary method, which are pretty good for ordinary primary batteries; but when the plate contains a lot of little holes there is such an enormous surface exposed that the action of the current is apt to disintegrate the plate, and the face will scale off.

A better way to make the plate is then to be considered.

There is a method of making these plates which is very much liked—a method in use by the United States Electric Lighting Company, in the manufacture of carbon for their incandescent lamps. That is, to take celluloid and put it through some chemical operation which deprives it of a part of its gum, thereby leaving a substance very easily carbonized. Another method I have found to work very well, and I shall have more to say about it in the course of a few months. I take coal-tar. This coal-tar, when it is evaporated and brought down to a requisite consistency, which is a matter of experience, is taken and put into a box prepared for the purpose and put into a closed oven. When it has reached a certain boiling point—and that is a matter also of experience, depending upon the exact state of the tar when you put it in—the heat must be quite suddenly increased by means of a double oven, one oven being heated moderately and the other to quite a high degree. When you suddenly increase the heat the tar no longer boils, but is carbonized.

Take a very large piece of this material which is carbonized just in that boilinge ondition, and a more beautiful sample of a porous substance can hardly be found—pores the size of small peas. This carbon is pretty hard and wears out a saw rapidly in cutting it; so I have devised another method of preparing the plates of a proper size which do not need cutting. The method of manufacturing, which I have described is susceptible of considerable uniformity. The character of these plates is porous, and they are of considerable value in a secondary battery. They are as permanent in an acid as glass is in water. I think the secondary battery of the future is to be composed of two carbon plates, made something in that way. I do not assert that that is a better way than putting mechanical holes into the carbon, but it is considerably simpler and costs less.

Planté is, of course, the great originator of secondary batteries. A Planté cell will return 40 per cent. of the power put in, and even more, if it is properly charged. A Faure battery will return, when carefully made, 90 per cent. It does not always do it, but it will return, under certain circumstances, 90 per cent.

The charging of a secondary battery must be carried on just as the ordinary work of electroplating is carried on. If you have two plates and wish to coat one with metal (it may be silver or anything else), if you use too high an electromotive force you deposit the metal in large flakes upon the plate. It looks as though there had been a snow-storm, the flakes coming down of an enormous size, and the consequence is it peels off. If you do the plating slowly and regularly, with a certain amount of current per unit of surface, you get a nice coat of the metal. So with the secondary battery; if you have two plates of a given size you will want to charge each with an E. M. F. that gives a definite electric current per unit of surface. If you have half a dozen plates you will want to increase the force half a dozen times. That is the whole secret of charging a secondary battery. Charged with care, I think there can be no doubt but that these plates will give all you desire in a secondary battery. As to their duration, I think they will be found good for five years.

Mr. SPRAGUE. With regard to the 90 per cent. of return I should like to know what parts of the circuit Mr. Koyl includes. Is it including the battery or excluding it?

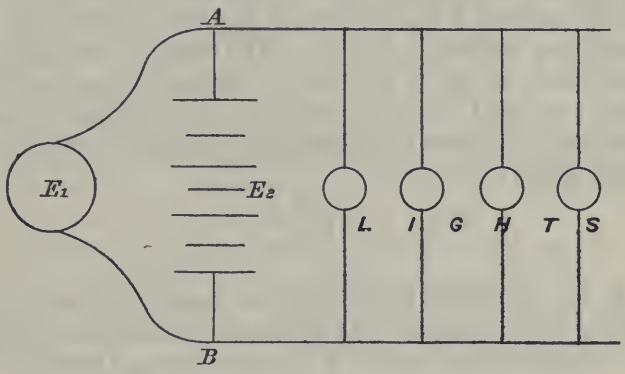
Mr. KOYL. Including the battery, of course.

Mr. SPRAGUE. Mr. Preece spoke of a case in which a dynamo is put in circuit with lamps, and also a storage battery, the effect being that the dynamo may be driven as irregularly as you please, and that the storage battery has a tendency to equalize the consequent irregularity of the current. The only reason why it will equalize the irregularity is very much the same reason that when one, two, three, or four dynamos are running in parallel circuit you have a light that is perfectly steady.

With a gas engine there is considerable difficulty in starting. I am speaking of one now, of about 3 or 4 horse power. A person has to put his foot on the fly-wheel and pull it around before he can get it started. If he simply uses a secondary battery he can light his gas engine and it will start ahead all right.

Professor FORBES. I think it best, where we have no difficulty about theory, to put it into proper language, and then we can see exactly what we are dealing with. The action is exactly as has been described, and I just want to show how the equations come out, because they are very interesting in showing the regulating action of a secondary battery upon the lights.

Let E_1 be the electro-motive force of the dynamo; r_1 the resistance of its circuit; E_2 the electro-motive force of the secondary battery; r_2 the



resistance of its circuit; r_3 we will call the resistance of the electric lamps in the circuit. Let us call the potential of that point (A) e_1, and of that point (B) e_2. These are the two points where all the three circuits unite. The current circulating in each of these three circuits can be put down immediately in terms of these quantities. Suppose that e_1 is greater than e_2, and take the direction of the current as positive when it flows from e_1 to e_2. The current which is circulating in the dynamo circuit is,

$$C_1 = \frac{e_1 - e_2 - E_1}{r_1},$$

The current in the secondary battery is

$$C_2 = \frac{e_1 - e_2 - E_2}{r_2}$$

The current through the lamp circuit is

$$C_3 = \frac{e_1 - e_2}{r_3}$$

It is a well-known law that the total current which passes through any one point is zero, *i.e.*, $C_1+C_2=0$. In this equation e_1-e_2 is the single unknown quantity. The result on working it out is that

$$e_1-e_2=\frac{E_1r_2+E_2r_1}{r_2+r_2}$$

This is a very useful formula and probably is well known. The application here is to finding the current which flows through the lamp circuit, viz,

$$C_3=\frac{E_1r_2+E_2r_1}{r_3(r_1+r_2)}$$

Attention has been drawn to this, that however irregular the dynamo may be, the current is very steady in the lamp-circuit when we use in connection the secondary battery. The reason is that the secondary batteries which we are in the habit of using have an extremely small resistance, compared with the resistance of the dynamo or lamp circuit. If we take the resistance of the secondary battery to be zero, our equation simply gives us,

$$C_3=\frac{E_2}{r_3}$$

That is to say, the current is exactly the same as if the dynamo were not working at all, and as if we had a battery of infinitesimal resistance, with this electro-motive force of E_2 working through these series of lamp circuits.

It is very satisfactory to hear from Mr. Preece that he practically gets over the buckling. I do not think any separator of which he speaks, would be sufficient to prevent the buckling of a Faure-Volckmar accumulator, for the force generated in these plates tending to buckle them is something enormous, and no ordinary separator would be able to resist this. The reason why he has succeeded in getting rid of this buckling is because he has reverted to what is evidently the most scientific, and which will ultimately be found to be the most practicable form, that is the old Planté battery. The objection to the Faure-Sellon-Volckmar is not only in the plates tending to buckle, but also, want of homogeneity in the symmetrical planes of the surface, which tends to induce local action. That defect was pointed out some time ago. Also, the minium will tumble off, and no satisfactory means has been devised to cause it to adhere.

I wish Mr. Preece or some one present could give further information about the length of life of these cells, because, although he is willing to use them in his charming house, even though they last only four months, it is not everybody who will. As a matter of fact, the later form of

Planté battery, the improved type which I believe is treated with nitric acid in order to render it more powerful, has been largely used only within the last year or two; and therefore its absolute life is not determined. I believe it is perfectly conceded that these Planté batteries, are far longer lived than any artificial batteries, such as the Faure-Sellon-Volckmar.

Professor VANDYCK. I do hope that people will not be discouraged in working in the line of other batteries besides the lead one. My own work satisfies me that the zinc, alkali, and copper battery will fill a considerable want. For instance, in running the magnetic apparatus in observatories and work where long life and retention of charge without loss for a great length of time is desirable. I have made a small battery of that kind, using a spiral of copper above and putting amalgamated zinc below so as to avoid the falling down of the deposit of zinc upon the copper, thereby amalgamating the copper and rendering it far less efficient. Although the electromotive force is only 97 or 98 hundredths of a volt, still we found by actual trial that the battery retained its charge perfectly for two months, which I think is a good showing. I was anticipated in the public statement that there was no action of the alkali upon the oxide of copper. Still I was delighted to find oxide of copper impervious to the action of alkali.

The only difficulty which I found in using the battery was a rather curious one—that we had to be very particular about the electromotive force to be used in charging it. The electromotive force is somewhere between a Daniell and a Grove. Charge a cell of that kind with a Grove cell and continue it, and the copper strip will be altogether dissolved by the alkaline solution, forming the cuprate of soda or potash, according to the alkali used. In charging with the Daniell cell there is no such difficulty experienced.

In experimenting I found that one could use a considerable amount of chloride of sodium so as to bring down the resistance of the cell. The resistance can be reduced very low indeed, and such a cell as that could be maintained, so far as I can see, indefinitely. The one precaution that is necessary to be taken is merely to throw a heavy mantle over the top to shut out the air.

I think that this matter of the copper battery is worthy of passing mention. I am positive that anybody who tries it made in the usual way with amalgamated zinc and alkali, either soda or potash, will be pleased with its properties. Preference may be given to soda on account of its being cheaper. The method should be to use plenty of chloride of sodium in it. I think the tendency of the copper to dissolve and to break up into cuprate was not any greater when the soda was used.

I want to say a word on the carbon question. I am interested to know whether anybody has succeeded in producing oxygen on carbon to any considerable extent without disintegrating it. In using a carbon

plate and trying to deposit oxide of lead upon it electrolytically, I have found that it would lead to the disintegration of the carbon by means of the oxygen. Hydrogen does not do it at all. The oxygen seems to have the power of accumulating in the pores and crowding out the carbon more than the hydrogen does.

Mr. KOYL. I wish to inquire of Professor Vandyck how his carbons are made. If formed by the ordinary method out of coal tar carbonized, they disintegrate in the battery. But if formed in the manner I speak of, from coal tar pure and simple, they seem to form a very much more homogeneous mass, and there is a striking difference in the permanency of the plates.

Professor VANDYCK. I will state that I used the commercial carbon simply.

Mr. KOYL. I think that there is a great deal of difference between the secondary battery composed of carbon plates and one composed of lead plates. I do not see myself the reason for such an expression of opinion, that the Planté battery is more scientific than other batteries—a Faure-Sellon-Volckmar battery, for instance. A Planté battery of sufficient size to last any length of time will make a secondary battery of enormous weight, and for any other purpose than putting down in a cellar to stay, the weight is a greater factor. Carbon plates will make a difference of about 80 per cent. in the weight, and will remain permanent so long as the box will or anything else about it.

Mr. ELIHU THOMSON. I should like to indicate a method which will tell whether the plates that receive the deposition of peroxide of lead will disintegrate or not. Suppose we locate one particle of oxide on the surface of the lead, what will be the electro-chemical conditions of that particle? Evidently the peroxide itself is in an electro-negative condition, far more so than the lead. Consequently it will set up a local circuit and distribute its oxygen more or less to the surrounding lead, and thereby eat into the lead deeper and deeper, destroying the life of the battery in time. It would seem from this consideration alone that the lead battery is certainly limited in its life.

Let us take the case of the carbon plates. Has it yet been proved that carbon is stable in the presence of peroxide of lead? Perhaps it may disintegrate the carbon into oxide of carbon and at the same time reduce the peroxide of lead to the protoxide of lead (PbO). If these two substances differ in electromotive force ever so slightly, we will have a local circuit which will take oxygen from the peroxide of lead and oxidize the carbon. If we wish to make a test of that, let us take a massive piece of peroxide of lead attached to a specimen of the carbon, and put in the circuit means of testing a feeble current. Put this in an electrolytic liquid—for instance, sulphuric acid—and see whether we have a difference of electromotive force between those two substances. If there is no such difference, then the stability of carbon in contact with peroxide of lead will be established.

It may be that the states in which carbon is known to exist may control the matter to some degree. The artificial carbon, which undoubtedly must be of a porous nature, would naturally be expected to disintegrate more readily than carbon which has been coked into a porous condition. Again, it is well known that if we take a stick of carbon and send a current through it of a sufficient degree to almost vaporize the carbon—in other words, to make it almost as hot as the carbon in an electric arc-light—it undergoes a change of condition into the graphitic variety. Whether it would be suitable in that form is another question.

Professor HOUSTON. I wish to call attention to some experiments made by Elihu Thomson and myself prior to 1878, in which we used two plates of copper, sulphate of copper being deposited on one of the plates and metallic zinc on the other. We obtained excellent results.

Lieutenant J. E. JEWELL. In reference to secondary batteries, I have had an experience of six months with one form of storage battery, a commercial battery used for lighting forty incandescent lamps. We have only ten or twelve of these lamps in use at any one time, consequently the demand on the battery is not very great. We find that with a 20-ampere current, instead of having to charge the battery all day, as I believe would have to be done in case the whole forty lamps were to be used, we charge for two hours in the morning, or until the cells give off gas from the plates, when we consider the charging as completed. I believe, after three weeks' use of this battery, it was observed in the first place that the lamps were not up to their usual standard of power. The next morning it was found that certain of these cells, four or five of the twenty-one that formed the battery, were much lower in electromotive force than the other cells of the battery. It was thought these cells might have become exhausted by short circuits, but close examination revealed no short circuits in the cells, and, furthermore, when the charging current was put on again those cells were the first of the whole battery to give evidence of being charged. In other words, it would seem as if the storage capacity of these particular cells had been diminished in some remarkable way.

We restored those cells by a prolonged charging of some twenty hours with a current of 20 amperes, and then a discharging of the whole battery on a fixed resistance, which was about equal to the lamp resistance, say, .8 of an ohm; then another charge of twenty hours, with a 20-ampere current, and again discharging the battery for three or four hours on this fixed resistance; then recharging for twenty hours. At the end of that time the battery was in good condition again; but in every case, I think, in about three weeks these batteries gave out. Out of five or six cells four have been involved every time, and I think it is only a question of time when all the cells will behave in the same way. The whole resistance of the battery is very low, and the electromotive force is very constant, with the exceptions mentioned, and the light is very satisfactory. But I am not at all sure that Mr. Preece has found a so-

lution of the lighting question by means of the secondary battery. It is to be hoped that he has. My experience rather indicates that we have not got a storage or secondary battery which is practical.

Mr. PREECE. I should like to corrobate what Professor Carhart said about the percentage of energy we recover from the storage battery and utilize in shape of light. Very recently Professor Dewar was kind enough to make a calculation for me that brought out a very interesting result. I wanted to know the relative proportion of energy expended in different modes of artficial illumination. The result of Professor Dewar's calculation was to show that in a sperm candle for 1-candle light we expend 97 watts. The experiment with gas showed that for every candle given out 62 watts were consumed. I have been experimenting with various incandescent lamps, and one lamp gave a result showing that it was possible to obtain one candle for 2.5 watts, while with arc-lamp illumination, such as we use in the street, we get a candle for each watt expended, and if the arc-lamps were bereft of the hideous structures put around them to reduce their light, we should probably get a candle for each half watt. That shows the proportion of 1 per cent. Professor Carhart named must be within the mark. For if we can get one candle for the expenditure of 1 watt, and we otherwise expend 97 watts in the process, it is clear that 96 watts must have been wasted.

I believe the days of the arc-light are numbered, and that all the light in the future will be furnished by the incandescent lamps. Up to the present time in the incandescent lamp the light is produced by the expenditure of from 4 to 5 watts per candle. Very rapid improvements are being made in the form of the carbon filaments. We are now using lamps in England that give us light with an expenditure of only 2.5 watts per candle. If improvement goes on at this rate I am quite certain that before another decade we shall have incandescent light that will give us one candle for each watt. When that is the case, then the incandescent lamp will be used in the place of the arc. The arc requires constant personal supervision and mechanism to keep it in order, and only lasts a short time. The incandescent requires no attention whatever after it is put up, and its life is very considerable indeed. In London lamps can now be obtained whose life is guaranteed to be a thousand hours. If we can get incandescent lamps giving light at the expenditure of 1 watt per candle, and whose life will be over 1,000 hours, then it will be a case of good-bye to the arc lamps.

There is one point I did not mention in regard to my battery, and that is I am using only 17 cells and 30-volt lamps. First, because it gives me very few cells to keep in order; and secondly, because the electromotive force is so low that there is not the slightest fear of shock, or of fire due to a short circuit or imperfect insulating means. In isolated houses the lower you can reduce the electromotive force, the safer it will be.

I differ altogether with the view that has been expressed that carbon

is likely to replace lead for secondary batteries. Carbons do disintegrate with us in London to a very large extent, in the present form of battery. The battery we principally use is a bichromate battery. In that battery the carbons do not last more than twelve months. They do disintegrate; they tumble to pieces; they become quite soft and spongy. We are obliged to use cut carbons, because the molded carbons will not last more than two or three months. While the cut carbons do not last more than twelve months, the lead in the secondary batteries will last more than twelve months. I did not make the statement that the lead lasted only four months. I think it will last more than twelve months. I know of houses where the batteries have been in use more than twelve months, and I am quite satisfied that the lead used in the one I have described will have a durability of more than twelve months. I shall not be the least surprised if it lasts for two or three years.

Mr. Sprague referred to the use of the secondary battery for starting the gas-engine. I look upon the employment of such a battery for such a purpose as simply barbarous. The greater portion of the causes that have brought secondary batteries into such ill-repute are due to the various barbarous practices that have been adopted by those who have been using secondary batteries. These practices have been indulged in without the person knowing the injury done to the battery. The practice has been to dash in a piece of wire in order to see the sparks, and then to say "what splendid order the battery is in." A more iniquitous or sinful practice could not be adopted. Such a practice as that has given the poor battery a straight blow between the eyes, with which it will struggle for days. Any practice that suddenly calls into action the force of the battery, such as short circuiting, tends to destroy it. I test every cell of my battery every morning with a galvanometer. It takes but a short time, and I see exactly the condition of the cells without in any way interfering with the condition of the battery.

There is one very peculiar defect of the Faure batteries which follows from covering the plate with minium. It is the formation of trees on the plates, bringing the plates in contact. That is not observed with the Planté battery. Planté himslf has never suffered from the formation of trees, nor have I in the batteries I am using seen the slightest sign of these trees. Such a grate as I use to prevent buckling would not prevent treeing, and therefore it will not cure that defect, which is one of the most serious defects of the Faure battery.

I mentioned that there had been a good many untruths expressed about secondary batteries, but the untruths about secondary batteries are as nothing compared with the untruths about primary batteries. Such a battery as we have had described to-day has been brought before the London public, and it has been stated that the products of the battery formed in its action will more than repay the cost of the battery. Therefore it has been suggested if that is true what a splendid

thing it would be for the Government to buy up all these batteries, use them, and in that way pay off the national debt.

The inequalities to which Lieutenant Jewell referred are due to the impurities of the liquid. I have suffered somewhat in the same way. I have cured it precisely in the same way by putting on the power for twenty hours, and in that way the impurities, or whatever they may be, have been worked out. The result has been that after doing that twice I have had no difficulty for the last two months. Let me tell you what I propose doing: I am going to have my battery in such order that my gas-engine by going all one day will charge it sufficiently to enable me to keep my house lighted for the rest of the week.

Professor JAMES DEWAR. My view with respect to secondary batteries, so far as I have been able to look into the question from the chemist's side, is about this: I feel that in the future some other body than peroxide of lead will be discovered, which will more efficiently represent the amount of energy absorbed. I take it to be that after all it is a question of the relative efficiency of the agents which have been, we may say, disassociated with the electrolytic action.

It seems to me that apart altogether from the difficulties of conduction and the question of local action, that the question is, Is it not conceivable that possibly a large class of bodies can be formed which will be highly efficient, provided we arrange to prevent local action, as we must, to a large extent, in other cases?

Take the case of the types of these bodies. We know that in ordinary cases of decomposition, even of the simplest kind, we have two actions taking place. Take the case of the formation from potassium and chlorine of chlorate and chloride of potash; as a matter of fact, we have an exothermic and an endothermic action taking place side by side. This action takes place, like a majority of chemical actions, with a considerable evolution of heat; but the evolution of heat is, in this case, due to the formation of the chlorate and not to the formation of the chloride. The chloride itself would be endothermic or there would be a reduction of the temperature. The energy for the formation of the chloride is really in some miraculous way extracted out of the energy produced by the direct formation of the chlorate. This is the type that we want in electrolytic action. In the electrolysis of water, peroxide of hydrogen occupies the same place as chloride of potassium in the reaction. In the case of the electrolysis of muriatic acid, hypochlorous acid occupies the same position; it is endothermically formed with a considerable absorption of energy, and consequently can decompose again with the evolution of heat. If we, on the other hand, replace chlorine by iodine, we find in the formation of an iodide and iodate that there is, if anything, a slight reduction of temperature. That is because the iodide and iodate are both positive. It would be impossible to form iodate if iodide had been formed by the absorption of energy.

If we could construct a battery in which all the oxygen is fixed in

this way to an unstable body, there is no question but that we could produce a much higher electro-motive force. We know very well that in a great majority of salts the electro-motive force is practically constant. That accords with the well-known fact that the thermal value of the majority of soluble salts is nearly independent of both the acid and the base; that is to say, it is nearly constant, giving us something like 15,000 Gramme units. Therefore, if we construct a battery of soluble oxide and soluble acid, we may say that the electromotive force is practically constant, because the thermal value is practically constant. If we wish to get a higher electro-motive force we must produce a form of different type.

The chemistry of this lead battery is not by any means cleared up. It would seem that the reactions that the chemists have mistaken for peroxide of hydrogen reactions are really reactions which do not belong at all to the electrolysis of water, but are obtained from electrolysis of some variety of sulphuric acid not properly distinguished.

That being the case, we have to consider cases where we can have converse reactions easily. A type of that kind would be of this order: Take iodic acid; that may be easily formed by the oxidation of hydriodic acid. We may very easily get hydriodic acid. Hydriodic acid may be formed from nascent hydrogen and iodine. Only in this case there is undoubtedly a considerable dissipation of energy, because, unlike chlorine, this change takes place with a considerable evolution of heat. The moment we mix the hydriodic acid with the iodic acid we simply reproduce again three of water and three of iodine. It is reactions of that type that we require. For the same water and iodine here, we have the hydriodic and iodic acids, and these come back to water and iodine again without any possible derangement. It is to the question of getting reactions of this kind without dissipation of energy that I look in the future for further advance in the construction of secondary batteries.

With reference to the disintegration of carbon, the same thing is remarkably apparent in compressed graphite, even when produced under great hydraulic pressure and when perfectly pure. If it is used for the poles of the battery in ordinary electrolysis, it degenerates with the most extraordinary rapidity. It is largely due to the mechanical action of gases of the battery on the graphite. The carbon does disintegrate, and the chemical compounds formed seemed to be of the same type as if they were produced at a high temperature. The chemical product seems to be what is called mellitic acid. It has carbon added, as it were, with 6 of carbonic anhydride, viz, $C_6 (CO_2H_6)$. It is of the same type as benzole. In fact it breaks up into carbonic anhydride and benzole; the result of the electrolysis being ultimately mellitic acid and gases of carbonic anhydride and benzole.

With reference to Mr. Preece's remarks in regard to the efficiency of the incandescent light, although it has nothing special to do with sec-

ondary batteries, except in their application, it is quite clear that, if what he says is true, and I do not doubt it for a moment, he must be working with an incandescent filament at a very much higher temperature than is customary. If we do not reach the point of destruction of the carbon there is no reason why the efficiency of the incandescent lamp should not be as great as that of the arc. Some years ago I made a series of experiments as to the increase of the amount of luminous intensity with temperature. I used platinum within a considerable range. Since that time others have followed in the same line, and seem to prove that if you take 500 degrees as representing the temperature at which light begins to be emitted, then you may say that the mean rate of increase of luminous intensity is as the sixth power of the temperature over 500 degrees. That very fairly represents my own results, and also the experiments of others. This formula enables us to give the temperature of the incandescent filament; and there can be no doubt that this is the explanation of the increase of efficiency when you work with higher temperatures. Also the higher the temperature the greater the refrangibility.

There are a great many points in connection with the chemical side of the battery which we have been discussing which we are not in a position really to answer, but sufficient, I think, has been said to point out the direction in which we ought to work.

Mr. KEITH. I would like to ask Professor Dewar whether he has investigated, either theoretically or practically, the decomposition and recomposition of the sub-salts of lead, such as were the subject of discussion some time ago. I think he was not present when I stated my own experiments in that line.

Professor DEWAR. No; I regret to say that I have not.

Mr. KEITH. Taking the solutions known as Goulard's solutions of sub-salts of lead, which are all very soluble, the peroxide and metal are readily formed and dissolved and redissolved in the solution under electrolytic action. So far I have carried them into practical effect only in the laboratory, but I have found that all the electricity is returned as electricity, but of course all the energy applied is not returned.

Professor DEWAR. The only experiments I have made in that connection are of the same type as many others, namely, that if you take an ordinarily compressed block of pure sulphate of lead, place it on a platinum electrode, and electrolyze it for hours, you will only succeed in getting a very small quantity of peroxide of lead. As I understand it, the fundamental action is admitted to be that the peroxide of lead is really produced from the sulphate; I do not say it is not produced from the sulphate; all I say is, that if you take a compressed block of sulphate of lead on a platinum plate, and if it is electrolyzed for hours, you will get a small deposit of peroxide of lead.

Mr. KEITH. I think the gentleman misunderstood my question. I want to state here that from solutions of subacetate of lead there is by

electrolytic action deposited upon one pole peroxide of lead and metallic lead is deposited on the other pole. Reversal of the current in discharge reverses the operation; both peroxide of lead and lead are dissolved. There is a difference of potentials, say, of 1.70 volts.

The Conference then adjourned until 3 o'clock p. m.

On resuming at 3 p. m.:

Lieutenant JEWELL. I would like to say a few words on this subject, and point out some of the defects in storage batteries of the Faure form. In regard to lead building between the plates, I found that thing quite a common experience in my experiments some three years ago. I was rather sat on when I called attention to these experiments and to these facts at the meeting of the British Association at Montreal. But I believe that the same thing has been referred to by Gladstone and Tribe. The cells to which I referred were not Faure cells. They were cells of the Planté type, but of a peculiar construction. The lead plates were of a peculiar form, and were entirely separated from each other by pieces of paraffined wood to prevent buckling.

I am not inclined to agree with Mr. Preece on that point of the impurities of the lead. The lead, in the first place, was as absolutely pure as lead could be obtained. Beyond that the plates were so placed that if the lead was not pure lead any local action that would take place in the cell on account of the impurities of the material would not affect the electro-motive force of the cell; it would cause a waste of the material, but would not affect the resistance of the battery nor its electro-motive force.

I attribute the action to which I referred to the formation of some non-conducting film over the surface of the plates, both the peroxidized plate and the metallic lead plate. There is no doubt that the oxide of lead, in the particular form in which we find it in the secondary battery, is attacked by sulphuric acid, with the formation of sulphate of lead. We all know that sulphate of lead is a pretty fair insulator. The fact that the cell, after prolonged charging, during which, of course, there must be a great loss of energy, is restored to its original condition, induces me to believe that it is not impurity in the lead, but rather the non-conducting film which covers the plate. I am quite satisfied, from attendant circumstances and from observations during experiments that were made, that there was no short circuiting in the cells.

I am glad that Mr. Preece has had such an encouraging experience with storage batteries; but I am inclined to think he will be disappointed in the hope expressed of being able to charge the cells on one day of the week and of then using them during the rest of the week. A very simple calculation would convince him, as well as the Conference, that such a plan is not practicable with his present plant. Perhaps if he increases his plant he may be able to do it. If we take the figures as Mr. Preece has given us of one candle from 2.5 watts of en-

ergy, then a 16-candle lamp requires 40 watts. Forty watts with 30 volts difference of potential between its terminals gives us 1.3 amperes as the current to each lamp. I assume that Mr. Preece has 10 lamps, which will require 13 amperes to maintain them. Thirteen amperes for three hours on an average night gives 39 ampere hours, and for seven nights in the weeks 273 ampere hours. Divide 273 by the efficiency of the storage batteries, 40 per cent., and we get, as the total number of ampere hours with which the battery must be charged, 682½. Dividing that by 12, the number of hours during which the charging will take place, will give us the current to be used in charging the storage battery to produce this result, or 56.9 amperes. Mr. Preece stated that his dynamo gave 52 amperes with 42 volts, or 52 amperes against a charging resistance of .8 of an ohm. I call the charging resistance of the battery the difference of tension between the terminals of the battery divided by the current flowing through the circuit. The charging resistance, I am sure, from my own experiments, cannot be less than 2 ohms. The current of his machine will be very materially reduced by that. Besides admitting that he could get this current of nearly 57 amperes, which he requires for charging the battery, the question is how long his cells will stand a current of that magnitude. I am inclined to think that a very few weeks' charging with a current so great as that will soon use up his cells.

Mr. PREECE. It is a dangerous thing to tackle figures; you can always make them prove anything you like. The assumption started with is wrong, for I am only using 10-candle lamps, and they absorb .9 of an ampere. I cannot go through with the calculation here. But experience shows that I can get 200 ampere hours out of my battery, so I shall have enough to last me a whole week. The batteries, I am quite sure, after they have been used a little time, will give me that and a margin to spare. I am quite certain, notwithstanding those figures, that if I have the pleasure of coming over here again I shall be able to report that from one day's charging I have had my light for a week. I will mention that I have had sufficient for four days with one charging, including two dinner parties, when, of course, one makes all the show he can. And as I did that in an emergency, I think I am not far wrong in saying that with a little gentle care that can easily be given, I shall be able to have enough for a week's use.

Mr. KOYL. I am afraid the public at large will think we have had a dinner party from some of the statements we have been trying to make. I would like to put on the board some more figures, even though they are dangerous.

I believe Mr. Preece said that in England it took 62 watts to furnish one candle power by the use of gas, and only 2.5 for an arc lamp. Some calculations made two or three months ago made me wonder at this. The calculation will take but a moment. I think that the result arrived at is entirely erroneous, and for the sake of the public at large, who,

thinking because we are assembled as an electrical conference, we are talking very high, I would like to present it.

Professor ROWLAND. There is no necessity of making the calculation. I suppose that the 62 watts and the 2.5 are in terms of heat. In the transformation of the heat through the steam engine all but 10 or 12 per cent. is lost.

Mr. KOYL. And instead of coming out 62 and 2.5, the two come out very nearly equal?

Professor ROWLAND. That is about it, taking that point into account.

Mr. KOYL. Thus gas can be furnished to-day with the same expenditure of energy and for almost the same price as can the incandescent light.

Mr. SPRAGUE. Mr. Preece accused me of a barbarous practice in starting a gas engine by closing the short circuit of a storage battery. I am not a friend of storage batteries and do not recommend their adoption; but I would suggest to a person who is going to start a motor in that way, that it is easy to shunt down to 1.5 volts of a storage battery. It would be a better practice to introduce a resistance in the circuit so that in passing the current through the dynamo no current could possibly pass that would damage the battery. Any one who has had the trials and troubles of starting a gas engine of from 2 to 40 horse-power by purely muscular effort will agree with me that if we can start the gas-engine, say, by sending part of our current back through a motor without damaging the battery in any way, it is a great boon.

Mr. PREECE. Let me remark that every gas engine I have seen has an extremely simple mechanism called a self-starter. A child can start the engine by pressing the tap by which a cylinder buried underground is put in operation. This cylinder takes a mixed charge of gas and air under pressure of about 100 pounds, and by the movement of a tap an explosion in the gas chamber develops sufficient power to immediately set the gas engine in motion; so there is no need of using secondary batteries nor any other means of starting.

Mr. SPRAGUE. I spoke of what had been done. At the time I spoke of, no self starters had been used. Mr. Schlumm, who was making the Otto gas engine, had not then made a self-starter. I do not think there is any gas engine in the United States that has this self-starter. Of course if anybody has a self-starter he can use it.

Professor FORBES. I want to ask if there is any one here who will tell us more about the testing of secondary batteries. It is a point upon which I have hoped to hear some remarks. In my opinion no scientific test has ever been made as to the efficiency of the secondary battery. In order to have the test efficient, we must know what we put in and what we get out. We must leave the battery in exactly the same condition electrically as it was when we started with it. I believe these conditions have never been fulfilled.

The PRESIDENT. The next subject is Induction in Telephone Wires,

Long-distance Telephoning, and Underground Wires. I call upon Mr. Lockwood to state some practical facts with respect to induction in telephone wires and so on.

Mr. LOCKWOOD. Mr. President and gentlemen, I find that the subject on the programme is entitled, "Induction on Telephone Wires, Long-distance Telephoning, and Underground Wires," a title that is very comprehensive and that is somewhat misleading, because from actual experience I have found but very few of the disturbances which we meet with on telephone lines are, in fact, due to induction. A better title is, "On the Electrical Disturbances Occurring in Telephonic Circuits and the Methods Proposed for Obviating or Overcoming Them."

Twenty years since, when electricity was a mere branch of physical science, it was easy for the student, the professional man, or the engineer to keep pace with its advances in every direction, for the ramifications of applied electrical science were few in number and comparatively simple in character.

When we have referred to the telegraph in its several forms, to electrolysis in its applications to electrotyping and plating, and to the miscellaneous practical operations of gas-lighting, burglar and fire-alarms, and to electro-therapeutics, we have substantially scanned the limits of the electrical field of that day.

At the present time, twenty years later, the domain of electricity has so wonderfully amplified, is even now so extensive, and is so rapidly enlarging its boundaries from day to day, that we cannot, and may not, dare to limit its possibilities, although we regretfully recognize the inevitable truth that it is no longer possible for a single mind to grasp fully, comprehensively, and in detail the multitudinous forms and the numerous subdivisions of the useful applications of electricity and magnetism.

Where, two decades since, one might by close attention, unremitting perseverance, and natural ability view the entire department to which I have alluded, and, Selkirk-like, pronounce himself "monarch of all he surveyed," now he must aspire in vain to such an eminence, for where at that period we could utilize electricity in forty ways, we now enumerate four hundred applications of the same form of energy without exhausting the list, and when the aspirant to universal and all-pervading knowledge views the vast circle of useful arts lying about his feet, each dependent in some way upon electricity or its affiliated forces, and each requiring almost the proverbial span of human life, three score and ten years, for the acquisition of a perfect acquaintance with its workings, he may well stand aghast and confess that such a universal knowlege is now attainable, and that if he achieves excellence in a few branches he may reasonably be satisfied with a more general and cursory acquaintance with the others. We often hear the expression "Electricity is in its infancy," and it may be interesting to note that this

phrase is not exclusively the sentiment of our own time or even of the nineteenth century. In a book which has recently come under my observation, entitled "An Essay on Electricity," written by Mr. George Adams in 1792 (and proving conclusively in the estimation of the author that electricity is a real and substantial matter), we find the same statement made in the following connection:

> As electricity is in its infancy, when considered as a science, its definitions and axioms cannot be stated with geometric accuracy.

The foregoing considerations have, in some measure, guided me in my choice of a subject, since I have reflected that only in a realm with which I have long been familiar can I hope to bring forward any thoughts which can in any degree interest such an assemblage as this. I apprehend that our present and rapid advance in the utilization of electricity is due, to a considerable extent to the more matured views which we now hold with respect to the character or nature of that mysterious and potent force.

For it is apparent that when by patient and diligent research we arrive at an intelligent comprehension of the nature of any force or element, we have made a long step towards realizing its capabilities; and when, in accordance with modern teachings, we assume that electricity, in common with light and heat, is a form of molecular motion, we have a warrant for believing that, notwithstanding the tremendous strides which have been made in applying these forms of motion to the work of satisfying the wants of mankind during the past years, and the allegation of Mr. Adams ninety-two years since, electricity, if not "still in its infancy," has certainly not passed the limits of childhood, and gives encouraging promise of a robust and well-developed maturity.

It is worthy of remark that each fresh discovery and invention in the realm of electricity serves not merely its direct and obvious purpose, but also performs the equally important function of a finger-post or guide-board pointing to further discovery.

One of the earliest results of the invention of the telephone, the practical speaking telephone of to-day, was the discovery of the universality of foreign electrical transfers; of currents passing constantly in ever-changing variety in electric lines of all descriptions. In other words the telephone proved itself to be an electroscope of the most delicate nature, which acts, as it were, in the manner of a microscope upon lines of communication, ascertaining and proving the presence of numberless currents, too small and infinitesimal to materially interfere with the coarser and comparatively expressionless telegraph instrument, but of an electro-motive force, volume, and variety sufficient to act powerfully upon the sensitive articulating telephone and to affect considerably its successful working. I have not the slightest intention to argue the vexed question of inventorship of the telephone, but I believe it due to myself to state that I hold it to be evidence of the strongest character that the various instruments produced from time

to time as alleged anticipations of the great discovery and invention of Alexander Graham Bell are not truly and really electric speaking telephones, except when used in the light of the present state of the art, in that they were not so far as we know troubled in their operations by induced or atmospheric electrical currents. Yet, almost coeval with the first published results of the articulating telephone of Bell, we find ourselves confronted with complaints that the noises which were not wanted made it very difficult frequently to hear and properly interpret the reproduced articulations which it was desired to hear. And almost immediately thereafter we find inventors racking their brains to invent remedies for these annoyances.

Some of the manifestations which have been experienced are most remarkable. It is scarcely necessary that I should attempt a detailed description of the various currents which surreptitiously introduce themselves into and haunt telephone circuits, as every one here must have noticed them.

The following notes concerning the behavior of these currents have been gathered from experience:

Long telephone lines are much noisier than short ones.

Sometimes one end of a long line (say 200 miles) will be perfectly quiet, and the articulations transmitted from the distant stations are reproduced with the most emphatic clearness; while at the same time the opposite end of the line is very noisy, and the attendant there finds the utmost difficulty in receiving the message by reason of the noise which drowns the articulation.

I have found as a general thing that lines running north and south are much more subject to disturbance than lines of equal length running east and west.

On the contrary, the individual line which has been most severely afflicted is a regular telephone line running east and west. This, however, is an iron wire of the large size of No. 4, B. W. C.

The sounds heard in a telephone on a noisy line are much intensified at or about midday or midnight.

Lines constructed along the sea-coast are, as a rule, much less subject to disturbance by induction than those which are inland.

Lines constructed on high land or in mountainous districts are subject to periodical storms—that is, at certain hours each day the noises reach a maximum intensity.

A line constructed of large wire is invariably much more subject to inductive disturbance than a line of small wire of the same length and strung in the same location.

A line of comparatively low insulation is not so noisy as a well insulated line of the same length, size, and in the same location.

A line consisting of a continuous metallic circuit is almost as noisy as an earth circuit between the same points, except when the two wires

of the circuit are parallel to one another, and equidistant from disturbing wires.

Now we know that the telephone currents proper are very feeble. It is therefore evident that an instrument, such as a receiving telephone, which can be acted upon by such currents must be correspondingly delicate and sensitive; and it is the very sensibility which I have already referred to and on which the legitimate action of the telephone is so dependent, which becomes under the pressure of the adverse influence of disturbing currents actually a detriment to the advantageous use of the instrument. It is simply this fact which has prevented any radical improvement from being made in the magnetic receiving telephone, for it is found that the capacity of the instrument to reproduce sounds, due to foreign and undesirable currents, increases in the same ratio as its capacity for reproducing the desired articulate sounds is improved.

It has, consequently, been found more profitable to direct our efforts to the improvement of the transmitter, whereby the telephonic current can be made more powerful and enabled to override the obstacles in its path.

If we attempt to classify the different disturbing sounds heard in the telephone, we find that they may be arranged as follows: (1) Frying, hissing, and whistling noises; (2) loud screaming and whistling noises; (3) jerking and rasping noises occurring occasionally; (4) telegraphic signals in course of transmission over other wires; (5) telephonic messages from other wires.

We may hear the first-mentioned sounds on almost any telephone, and at almost any time, their energy, however, varying with the length of the line and the time of the day. The sounds of the second class are chiefly heard about midnight and noon. I conceive that sounds of both classes are almost entirely due to the presence of earth currents, atmospheric electricity and thermo-electric currents. Earth currents we find most frequently to affect lines running northwest and southeast.

The free electricity in the clouds and air also appears to act inductively upon the wires (these being of course in contact with the earth), and produce a continuous series of small discharges, which manifest themselves by crepitating sounds in the telephone. I have sometimes listened all night at the telephone, and find all the above noises to be more intense by night than by day, the disturbances of the second class, *i. e.*, the screaming and whistling noises, becoming especially emphasized about twelve, midnight.

The thermo-electric currents I have mentioned seem to be developed by the varying temperature of the wire with respect to the earth, the said temperature being usually greater than that of the earth during the day and less during the night.

If this supposition be based on fact, no line in which the earth forms a part of the circuit can be totally free from these disturbing influences;

even if a complete metallic return be used, there will be at each point of support a leak to earth, albeit a leak of high resistance. A parallel metallic return is therefore not an absolute guard for these currents. The French philosopher, M. Gaiffe, has devoted much time to studying the various causes of these disturbances, and in addition to those I have named he puts forward the statement that "every change in the position of every wire relatively to that of other wires on the same support causes magneto-electric currents in the others."

This position is, I think, incontrovertible, and my own observations lead me to make the following addition thereto: "That when wires are suspended in a direction at an angle with the magnetic meridian, magneto-electric currents are developed in them with every lateral movement or swing which they make."

Referring to the third class of interfering sounds—jerking and rasping, but isolated noises—they may be attributed to mechanical defects of the circuit, such as imperfect joints and loose connections. Such imperfect contacts set up microphonic action, with its resultant sounds.

Since the remedy for this class of faults is so evident, I may as well advert to it now and dismiss it from our future consideration.

It is obvious that more care in making joints, splices, and screw connections and a judicious use of the soldering iron would effectually cure this disease.

We arrive at length at the disturbances which constitute the worst enemy of the telephone, viz, those which I have arranged in classes 4 and 5. Their perniciousness arises not so much from their loudness or intensity as from the fact that they infest short wires as well as long ones.

These are readily ascribed with certainty to a variety of causes, the principal of which are, leakage of electricity from other wires, electrodynamic and electro-static induction, and in the case of the fifth class, especially to the surcharging of small and inefficient earth terminals.

The first cause, leakage, is, as we are well aware, not exclusively confined to telephone lines, but has affected telegraph lines from the earliest times of telegraphy. Imperfect insulation allows the current to divide itself between all the wires on the support, in the ratio of their respective conductivities, and, as the shorter the wire the higher the conductivity, the tendency is generally to escape from a long circuit into a short one.

When the current escapes to the earth no great harm is done, as the only effect is to weaken the signals; but when it leaks into another wire it confuses the signals on the second line.

Dynamic or current induction also produces results, so far as the telephone is concerned, of the same general character, so that if the telephone line runs parallel to a telegraph line for some distance, the signals of the latter are perfectly reproduced in the former. I have my-

self repeatedly read off Morse signals in a telephone connected with a wire stretched adjacent to the Morse line. Worse yet is the induction between telephone wires, by which words spoken into the telephone on one line can be heard with equal facility in the telephones of other lines running in parallel proximity to the first. Electro-static induction, in addition to its retarding and prolonging results, which I shall have occasion to refer to hereafter, also exercises the repeating effect which I have described as accruing from current induction.

All of the disturbing agencies which I have noted may be observed in comparatively short lines. I have noticed them in lines of not over a quarter of a mile in length, but when a circuit is many miles long their operation is naturally greatly accentuated.

We may now consider the various means which have been proposed or used for the diminution of these vexatious parasites of electro-telephonic communication. As I have indicated, the necessity of some remedy was seen as soon as the telephone was used. Mr. W. H. Preece long since pointed out that three ways of attacking the enemy presented themselves:

(1) By increasing the intensity of the transmitted currents, so as to make them decidedly stronger than the inimical currents, and also to reduce the sensitiveness of the receiving telephone.

(2) To place the telephone wire beyond the range of induction.

(3) To neutralize or compensate the effects of induction.

But these methods all assume that the disturbances are due exclusively to induction, which we can by no means allow.

I believe that to intelligibly apply a remedy it is requisite that we should first be sure of the disease. The three hints are, however, good as far as they go, especially if we use them collectively.

We comply with the first by using the various battery transmitters and microphonic speakers, while in receivers the static telephone of Professor Dolbear affords a good instance of what has been and may be done in this direction.

The second method is incomplete, since it deals with induction only.

The third is the most promising. The earliest remedy suggesting itself to those who were interested in the subject was the most natural one, viz, the use of a metallic return wire. It is, however, a condition of success that the two wires must be parallel to one another, and so placed as to be subject alike to the disturbing influence. As this device was the first, it is also the most effectual, and is the only one which seems to attack and reduce the vigor of all the opposing agencies. It weakens the earth currents, as the only connection of the circuit with the earth is through the high resistance route of the poles. It diminishes leakage for the same reason. It opposes induction by receiving the induced impulses in both directions at once, and the induced current, like a two-headed snake swallowing itself, is neutralized by its own energy, and the effects of static induction are reduced, or at all events they are no-

ticed less, because the wire is otherwise comparatively clear and quiet. I would not be understood as making the statement that the use of the parallel metallic circuit for the neutralization of induced currents is a novel idea. On the contrary, it is a very old laboratory contrivance for the same purpose, and was patented as a remedy for static induction at a very early date in England by Dr. Warner Siemens. The application to telephony, however, is a new one, and has again brought it to the front.

Another device, tending to the neutralization of induction between any two circuits, was to provide a double-wound induction coil, and to pass the two circuits through the said coil reversely with respect to one another, so that the currents induced by one upon the other on the line would be neutralized by the same forces exercised oppositely in the induction coil. While in a measure successful, this expedient is not practical, for several reasons. When more than two lines are involved the apparatus becomes cumbrous. The resistance of the coil portion of each circuit is a dead weight upon the working of the line.

It was at this time ascertained by experiment that a multiplicity of telephone wires running together was actually an advantage. It was found, for example, that although when there were but two or three wires the interference was quite pronounced, the increase in the number of the wires greatly reduced the virulence of the trouble. The disturbing currents seemed to divide among the circuits, thus helping each one. This, of course, is in accordance with the law of derived circuits, and is the fact which has made the telephone exchange possible. It seems, however, to apply only to short wires. Subsequently it was assumed that the great part of the objectionable noises arose from actual leakage alone, and that induction or earth currents had little to do with it. Acting on this presumption, it was proposed to provide each insulator pin with a metal socket fitting in the cross-arm, to unite all the sockets on a pole by wire, and to connect them all with an uninsulated wire running from pole to pole, this wire in turn being united as frequently as was thought necessary with ground or earth wires leading down the poles. This also was an old idea, having been originally devised and patented in England by Edward Highton, and applied there to the telegraph poles from that time until now for the purpose of conducting leakage to earth. And this remedy indeed does reduce the total disturbances very considerably, inasmuch as it carries off the actual leakage. It is by no means a perfect remedy, and, in my opinion, by no means an unobjectionable one, since not only does it grapple only with such disturbing currents as have actually leaked by conduction from wires, but it also greatly enlarges the electro-static capacity of the lines, since it brings the earth virtually into close proximity to the lines, thus introducing the retardation or sluggishness and prolongation which always is the attendant of static induction, and produces indistinct articulation.

I assisted in the year 1880 in fitting a forty-four mile line between Boston and Providence with these contact conductors, and when finished, although the line was not very noisy, the retardation was quite perceptible and caused a kind of echo to the reproduced sounds, at the same time affecting the clearness of enunciation.

We then attempted to lessen this by placing at intervals of a few miles high-resistance leaks to earth. This worked a short time and reduced the capacity, but it was found difficult to maintain a constant resistance in the leaks, and ultimately the lines were coupled up as metallic circuits, and in that arrangement gave entire satisfaction. It is, I think, unwise to attempt to diminish leakage on pole lines in any way except by improving insulation, and even then it seems desirable to keep the static capacity of the line as low as possible. In fact one of the patended devices for reducing induction is to place at suitable distances from one another along the line a leak to earth, by omitting perhaps one insulator per mile, and fastening the line wire instead to an iron cap, which incloses but does not touch the terminal of an earth wire.

In 1879 compound conductor cables for aerial work were introduced to reduce induction between the wires. Each insulated conductor was sheathed in metal foil according to the patent of Dr. Foucault, the foil being connected by wire with the earth. This expedient also proved effectual, and has till the present time remained in use. In using this expedient it is assumed that the insulation of each conductor is good, so that leakage may be eliminated from the list of opposing agencies. It is evident that the arrangement acts as a screen against induced currents between wire and wire, acting thus as a preventative of interference from such currents, while it simultaneously weakens the intensity of the sounds due to earth currents by increasing the capacity of each wire. This, however, is effected at the expense, as in the pole lines, of distinct articulation, inasmuch as retardation is quite pronounced when the cables are more than a couple of miles long.

Such success as this device attained has brought forward a host of imitators. Among others, a modification of this device has been introduced by Mr. David Brooks, jr., of Philadelphia, in which two separate insulated wires were used for each circuit, one of the wires being included in the circuit, while the other was connected to earth.

A cable of solenoid form has also been proposed and patented by Professor Orazio Lugo, but it is of course only a variety of metallic circuit, having the leading out and return wire twisted together. These methods are all helps for short distances, but when long ones are required one consideration or another opposes their use. The worst feature about the earth-connected metallic sheath is the ensuing retardation, and experience has demonstated that a cable of twelve miles in length so protected is almost impassable to the electrical changes necessary to articulation.

I may also note that isolating the line from the earth altogether by

means of condensers has from time to time been tried, and while it is to a great extent efficacious against the earth-current annoyances, it does not seriously affect induction, inasmuch as, although there is no continuous sircuit, there is ample conducting area and surface for the variation in charge. The condensers also are very liable to be destroyed by lightning.

Van Rysselberghe, of Belgium, some two years since made experiments tending, among other results, to the freeing of the telephone lines from telegraphic induction. His plan was chiefly to so arrange the sending key of the telegraph system that it would, when operated gradually, introduce resistance into the line instead of altogether breaking the circuit, the theory being that the induction was caused by the suddenness and intensity of the variation in the telegraphic circuit.

There are two practical disadvantages in this arrangement, the first being that as a preliminary step it is requisite that both telegraph and telephone wires should be under the same control; a condition which rarely occurs; second, that so arranging the key cripples the efficiency of the telegraph service, as it would greatly decrease the speed of operation.

Within the last two years the construction of the compound wire of the Postal Telegraph Company between New York and Chicago marks a new era in telephony. It was ascertained, by actual experiment with that wire, that long-distance talking could be achieved with a conductor of sufficiently low resistance. It was at times a noisy wire, while at other times it was remarkably silent. But the success with which conversation was carried on over it showed that high conductivity was the principal condition, and that it was also a desideratum to combine if possible the requisite degree of conductivity with small surface, so that the electro-static capacity should be low. These conditions led the American Bell Telephone Company to fix upon hard drawn copper wire as the wire most suitable for long lines; by its use a high degree of conductitivity was gained, while the size was not enlarged, as would be the case if the same conductivity were sought in an iron wire. Heretofore copper had not been manufactured in a suitable way for this use, but at this time a good article of great strength had found its way into the market, and the American Bell Telephone Company built a double line between New York and Boston of No. 12 Birmingham wire-gauge hard copper. The line when erected was found to be an easy working one, but when either of the wires was used singly considerable noise, due to earth and atmospheric currents, appeared, making the conversation weak and hard to understand. The line has therefore always been worked as a complete wire circuit, and in that condition works quietly, clearly, and perfectly. We are thus brought back to the original remedy for the disturbing influences of telephone lines. It is the only remedy which has proved to be at all times efficacious, and we

are forced to the conclusion that with our present knowledge it is our best resource.

It may be interesting to state that when this double line was working perfectly between the two terminal stations, no sound being heard except the telephonic reproductions, should a way or intermediate station introduce its telephones and apparatus into one of the wires, so as to constitute a half-way station on one side only of the metallic circuit, noise at once appeared in the telephones at each station, the balance being evidently lost.

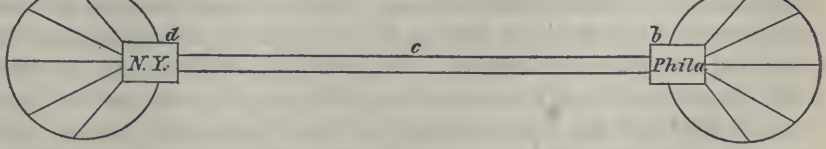
The features which oppose the use of the double-circuit expedient are obvious to every one; they are double expense of material and construction, and the difficulty of working it in connection with single earth circuits such as the great majority of our telephone lines are at the present time.

The former difficulty is of course insuperable, and will have to be borne; but it is probable that for such persons as desire to communicate frequently between cities at long distances from one another, loop lines will be built, adapted to form extensions of the trunk metallic circuits.

Thus it appears that while many devices have, from time to time, been proposed and explained, scarcely one is suitable for lines of all lengths, or is efficacious against all the forms of adverse currents.

In conclusion, we have apparently much reason to be encouraged to believe that in the early future, by carrying out only first-class construction, using conductors of high conductivity and low capacity (and we are aided in this by the knowledge that electro-static capacity increases, all things being equal, as the conducting surfaces, which only doubles with the doubling of the diameter of the wire, whereas the conductivity is quadrupled when the diameter is doubled), making only perfect splices and joints, uniformly soldered; better and larger ground terminals and improved switch-board contracts; employing powerful transmitters, utilizing metallic circuits for distances of over one hundred miles, and exercising judgment in proportionment of induction coils to line, we may be able to communicate telephonically over distances which only a short time since seemed incredible.

In the matter of metallic circuits between different centers of telephonic communication, I would like to illustrate upon the blackboard an invention which seems to me to be of some interest.



Suppose here are the central offices (*a* and *b*) at New York and Philadelphia, whence a number of lines radiate to subscribers' residences.

A double wire (*c*) connects these stations. It is obvious that connecting a single wire to the double circuit is of no advantage; it is simply making of the double wire a conductor of twice the size. We have grappled with some interest with the problem how to effect the connection so as to give the short line all the advantages of a metallic circuit without running two wires for each line. We have done it in this way: We have run a wire around from the central station to as many places as desire to use the metallic circuit. The subscriber by turning the button one way can talk out of town, and turning the other way he can talk in town; or he may have two telephones, which may be hung on the call-box, and understand that if he talks by the right-hand telephone he will be talking in town, and if by the left-hand telephone, out of town.

In the present stage of electrical communication, whether it be telegraphic or telephonic, the country seems to be very anxious to bury us underground. It is perhaps of some interest for us to know how far we can work underground. There is no doubt that different systems of telegraphy, especially those which are not particularly fast, can be worked for a reasonably long distance—certainly for cities—below the surface of the earth. In Boston we have a large system of telephone wires laid in tubes between vaults at street corners. Between two subscribers in the city they work fairly well, but not so well as those overhead. We have found that when we connect a short underground wire, of say half a mile, with a long wire, of say about 30 miles, leading out of the city, we have difficulty. The subscriber at the end of the short wire can make himself heard, but the subscriber at the end of the long wire is not in so fortunate a condition, and cannot make his friend hear him at all. To take an instance—I think the same trouble has been met in England in automatic Wheatstone lines—the subscriber at the end of the short line seems to be favored by the long line with a more even distribution.

Mr. PREECE. I regret very much to find that you have not in this country that advantage which we have in ours, namely, a society meeting very frequently where novel ideas are discussed, and where there is a free circulation of notions on work done in different parts of the country.

Mr. Lockwood commences his paper by saying that it was one of his duties to find out what has been done in these questions in other parts of the world, and I am bound to say that I think he has neglected his duty, insomuch that he failed to bring before you anything, as far as I can remember, of what has been done in England in curing the very defects to which he has alluded. Of course in England we are very much behind the world in a great many things. We are certainly behind the world in the practical application of call-signals and of telephones for the general transaction of business in large cities. I do not

know why it is so. I suppose it is the phlegmatic habit of the country. There is certainly some very strong reason why the telephone has not been adopted to the same extent that it is here. But if we are backward in the commercial use of telephones, we certainly are not backward in the scientific application and in the application of means to the discovery of the causes of defects in working, and in devising means to cure those defects.

If you had a society such as I speak of, I think it is highly probable the occasional papers that are read in our English societies would find some circulation here; and certainly a paper that I read in the year 1878 before the Physical Society in London, and a paper which was read by Professor Hughes before the Society of Telegraphic Engineers in London, and discussions that have taken place at those two societies, as well as discussions that took place before the Paris Congress, must have reached here and must have received some attention from you.

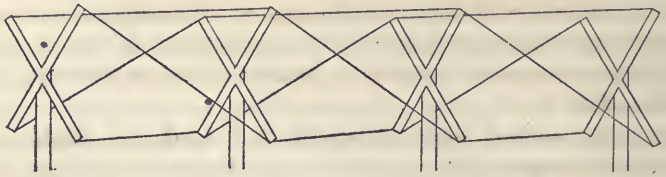
In the first place, I may say that I perfectly agree with Mr. Lockwood that too much attention has been given to induction as a cause of disturbances. Induction is not always the cause of disturbances. The disturbances are attributable to other causes, such as he enumerated.

Let us take one or two of them, not in the order he gave them, but just in the order that they struck me as he referred to them.

In the first place, as to the difference in the disturbances due to the geographical direction of the line, they have been observed in England, and I, like Mr. Lockwood, have spent nights in listening to these various sounds, and peculiar sounds they are. While holding the telephone to the ear, the curious whistling noises they made sounded very much like young birds in their nests. I think the cause is very obvious, and has been mentioned by Mr. Lockwood. It is due to the swinging of the wires across the lines of force of the earth's magnetism. It is very easy to comprehend that if a wire swings at right angles to a line of force a current will be induced; and when you have one or two hundred miles of line, with some twenty or thirty sags to the mile, there will be periodic swings that will produce musical notes and different sounds.

The very first telephone that was taken to England was taken over by myself. When I was visiting this country seven years ago, in 1877 I think it was, Professor Bell presented me with two telephones. I took them over to England and appeared before the British Association, then meeting at Plymouth, and there we spoke between Plymouth and London. The very first telephone circuit that was put up in England was a metallic circuit put up by me especially to cure this disturbance to which we have alluded. Although Mr. Lockwood, in his remarks, has spoken of a metallic circuit as being a cure for these disturbances, he has not hit the right nail upon the head, according to my belief; and that is, that the metallic circuit cure is not so much due, *per se*, to the

metallic circuit as to the fact that this circuit is made to pass around itself. When we put up four wires we put them up in this way:



If you could fix your eye at one end you would find the wires numbered in order, 1, 2, 3, and 4, upon the four posts, as follows:

1	2	4	1	3	4	2	3
4	3	3	2	2	1	1	4

1 and 4 form one circuit, and 3 and 2 the other. These four wires are constantly revolving around each other, two wires always keeping the diagonal position. The object of that is that, however many other wires there may be on these poles—in many instances as many as 20, 30, and 40 wires—the mean distance of each telephone circuit from the mean resultant wire of the other working circuits is always the same, so that you have a constant equalization of the influence of one wire upon the other. That principle is carried out to a very large extent between Liverpool and Manchester, between Glasgow and Edinburgh, between London and Liverpool, and between London and Manchester. And in future all the lines in the country will sooner or later be connected in this way.

The average number of wires on the poles is something like fifteen, and these are on the same poles which carry telegraphic wires. There is a modification of this plan being carried out of making the same arm carry two telegraph wires and eight telephone circuits.

There are two main features in this circuit, one is the total elimination of the influence of the earth and the other is the total removal of the inductive effect between one wire and another.

Copper wire is now used almost universally for telephone and telegraph purposes in England. The days of iron wire for these purposes are practically numbered. The copper suppled is of the strength of steel, and it cheapens the construction of telephone and telegraph lines to a very great extent. It is hard drawn copper wire with a conductivity of 90 per cent. of pure copper.

We have in the use of earths far more serious difficulty than Mr. Lockwood mentioned, and that is bad earths. It is astonishing what a tremendous resistance these earth-plates give. I asked one of the most distinguished electricians in France, M. Lavier, what allowance he made for the resistance of earth-plates in a good-earth-plate. He replied that he was quite satisfied if his earth-plate gave a resistance

of 25 ohms. We have considered that an earth-plate should give no resistance at all, but I have never yet succeeded in finding one that did not give any resistance. It is the resistance of these earths, so often carelessly made, perhaps a wire whipped around the gas pipe or a water pipe, so frequently used in renters' offices, that has, I believe, caused as much disturbance as any other.

Mr. Lockwood has a new and ingenious plan by which a single-wire system can be worked with the double-wire system; but we have another plan in use in England, between Edinburgh and Glasgow and between London and Manchester, and that is, by induction coils.

Between the stations is a double wire (*a*); (*b b*) are angle wires, and (*c c*) induction coils. The double wire is in circuit with the secondary wire of the induction coils. The subscriber is in the primary circuit of the induction coil, and if he speaks through the primary wire, induction sends a current through the secondary wire to the receiver. It is a very ingenious system for working a single instead of a double set of wires, and is very efficient. I do not think it is so good as the plan described by Mr. Lockwood, because it introduces two induction coils, which are always a source of serious trouble. He referred to the system of Van Rysselberghe. That has been tried; but like many other systems of the kind there is nothing in it.

There is a plan I would like to point out by which the disturbance of the intermediate stations may be somewhat obviated, viz: If we want to put in a way station, instead of connecting it to one wire, as most persons would do, we connect it to both wires by bridging across.

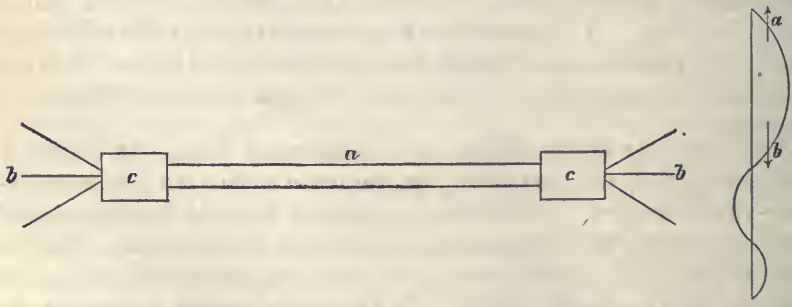
In a paper read at Montreal, Lord Rayleigh produced a formula giving the conditions for determining the distance through which one can speak through a cable, taking the Atlantic cable as his type. He showed that it was practically impossible to speak with ease over a greater distance than 20 miles. I happen to have made a great many experiments on cables between England and the Channel Islands, Dublin and Holyhead, and other places. I have also experimented largely on artificial cables. My conclusions agree thoroughly with those of Lord Rayleigh. It is possible to hear sounds through 100 miles of cable. It is possible, but difficult, to hear words at 50 miles. It is quite impossible to speak fluently and easily through a greater distance than 20 miles.

This leads me to the subject of underground wires. In England (I am speaking of the post-office), in the large towns, we use principally underground wires. In Newcastle, where we have the largest telephone system—about 600 subscribers—the whole is underground. The wires are covered with gutta-percha and made up four wires into a cable, and we have a metallic circuit. In this underground system at Newcastle there is not a disturbance of any sort or kind. There is no notice at all of the defects of overground wires except that rather serious one of the limited distance through which one can speak. With the

wires in present use conversation cannot be well maintained over a greater distance than 12 miles, the short distance being due to the fact that the wires are small. The simple mode of drawing gutta-percha-coated wires made up into a cable into iron pipes is an old system, which has been practically used in England for years. We have in England a very large underground system. We have an underground system between Liverpool and Manchester, and we have an underground system in all our towns. At the present moment we use nothing but gutta-percha for insulation. We have several systems on trial and there seems to be a wild mania all over the country to drive every one underground. The result is that every inventor who has failed to acquire credit this side of the water comes over to our side, and we are pestered with these inventors at the rate of about one per week. As a rule we give their systems a fair consideration, and when they are good we give them a trial. For instance, Mr. Brooks has an excellent system of underground wires, and we have laid them down for a distance of six miles out of London. Then there is Callender's, which is also be, ing tried here. I think there are one hundred schemes before the world- but I do not think that there are more than two which are worthy of consideration, besides the plan that is now in actual practice. We see no reason to depart from that, as it supplies all our wants. I refer to the common simple system of drawing gutta-percha-covered wires into iron pipes.

Professor ROWLAND. As Professor Fitzgerald has gone, and as I did not hear Lord Rayleigh's paper, I can only give you my ideas of what Lord Rayleigh might have said on that occasion.

I shall base my remarks upon the fact that the equations which express the motion of a wave of electricity in the wires are the same as those which express the internal heating of the earth, due to a periodic heating of its surface. As the earth is heated by the sun daily, the waves progress downward and gradually become less and less. It is just the same with the waves through a cable. Suppose the degree of heat is measured



from this vertical line to the right if above the average temperature, to the left if below the average temperature. The flow of heat is in the direction from the highest to the lowest temperature. So that the

heat is flowing in one direction along one portion (*a*) of the wave and flowing in the opposite direction along another portion (*b*) of the wave. Thus the waves become smaller and smaller as they go downward. It is the same in the telephone circuit. Some waves are conducted backwards as well as forwards, so that the waves diminish as they go along the wire. The law of that case is as follows: Supposing that this height is five times as great as the height of the next wave below—it is much greater than that usually—then that ratio will be the same for waves of all lengths; that is, the next wave above will be five times as high as this wave. Short waves, therefore, die out more rapidly than long waves, and the effect would be, after a certain distance, to throw out short wave lengths and leave merely the long waves. Therefore, a man with a deep voice could be heard much farther than a lady who had a very high-toned voice. As to how far one would recognize the voice, that is a difficult question, from a theoretical point of view. Lord Rayleigh calculated the distance at twenty miles at which one could hear respectably through a cable like the Atlantic cable. But I suppose the tone would be changed and the voice appear different. I am sorry Professor Fitzgerald is not here to give us the exact calculation.

Professor BROWN AYRES. In some experiments which I made several years ago, the results of which were published in the Journal of the Franklin Institute in the spring of 1878, I called attention to the fact of the great facility with which telephone messages could be exchanged along a line which was in a very leaky condition. I mentioned the case of two wires placed at my disposal in Virginia, one of which was well insulated, and the other on the ground for a large portion of the way. I found, much to my surprise, in trying the two lines, that although it was impossible to work the Morse system over the line which was on the ground, except occasionally, that I could always use the telephone very satisfactorily; whereas, on the other wire, a well-insulated wire, on poles all the way, I always had a great deal of trouble from this so-called induction.

I mentioned also in this paper the fact which Mr. Lockwood has mentioned to-day, that the line giving the most trouble as to working was a line running in the direction of northwest and southeast. I have tried a good many lines, and have found that these lines are worse than many others, and are subject to the daily periodical disturbances he speaks of.

Mr. Lockwood spoke of the fact of induction from telegraph lines. I have observed induction from the telegraph wire when it ran at right angles to the telephone wire without touching the same poles. I think that this phenomenon is the one referred to by Professor Bell, and discussed in a section of the American Association the other day, and is merely due to the fact that the terminals of the wires are placed in the field, and are at different potentials, in some way due to the current from the telegraph wire, so that the messages could be observed. I

had a striking instance of that at that time. I found on one occasion that I could read telegraphic signals which were being sent by the Morse system. I tried to find the arrangement of the lines around the surrounding country, and I found no line within one-quarter of a mile of the line I was working on. There was one short circuit within perhaps 200 yards. The lines that did run in the neighborhood were as nearly as may be at right angles to the wires on which we were working with the telephone. Even at this great distance, having ourselves a very short line, and taking the messages from a telegraph line of such length as the line between New York and Boston, the sensitiveness of the instruments was such that there was no difficulty in reading the signals. It seems to me to point out that some of the troubles of the Morse system may arise in that way when there is no direct leakage or induction or anything of that sort.

Mr. LOCKWOOD. If there is no more to be said by others I would like to make a few concluding remarks, as I have to leave for New York tomorrow morning. Mr. Preece, in his very gentle criticism on my remarks, stated as a preliminary that if it was my duty, as I declared, to know what was going on in the surrounding world in regard to the subject in hand, I had neglected it. I have much pleasure in declaring that whatever I omitted in reference to which he spoke afterward, I omitted deliberately. I did not think it necessary nor within the scope of this question to enumerate every remedy which had been proposed, or even every one that had been adopted.

With respect to the method of twisting the wires around one another, of which a fragmentary view has been displayed upon the blackboard, that was one of the first that was brought under my notice; and while sometimes it worked very well, at other times it did not work at all. The results obtained from it were by no means so pronounced as to compensate for the extreme difficulty incident to its construction and maintenance.

I perhaps have labored under some disadvantage from the fact that I was compelled to omit reading a portion of my paper. If I did not say, I certainly meant to say, that a metallic circuit was effective not as a metallic circuit, but only in so far as the two wires concerned were equally distant from the source of disturbance. Mr. Preece also stated that I omitted to mention one of the worst troubles about terminal earths. If my memory is right, I mentioned that one of the chief sources of trouble we had experienced was the overcharging of terminal earth-wires. I can, however, mention here that in large central stations where a number of telephone wires converge we find it readily practicable to dispense with terminal earth-wires altogether, letting each wire find its earth in all the other wires. It does not make any practical difference. I think sometimes it works a little better. Where, however, two-thirds of the wires may be engaged at once, the joint resistance of the others is not always a sufficiently low resistance.

We have also used the induction-coil method of repeating from a metallic circuit to a single line. I tried it in the autumn of 1879. As he says, we can talk upon a single line at one end, and repeat our words over the double line on to any number of single lines. Results in that case were not very good. We in this country have to cater to a great extent to the public mind, and when the public mind wants a telephone it wants a good one, or it does not want any.

I cordially agree with Mr. Preece in what he said about the length of cables over which it is possible to work. I have tried various lengths of subterranean and subaqueous cables, among others a cable from Newport to Block Island, which I think is 12 miles long. While it is possible for persons who have experimented considerably with telephones and telephonic circuits to understand what is said between the two ends of that cable—which is a very fair one, I think it belongs to the Government—it is by no means as easy for persons who only handle the telephone as a means of conversation between friends and in business to understand. We have to make a distinction between practicable and practical. It is, perhaps, wider in business than we should make it in science.

In respect to the point noted by Professor Rowland, that most likely the voice of a lady would not be heard so far over a cable as the deeper tones of a man, I may say that practice bears that observation out, that is to say, the female voice is better adapted to short circuits, and it is with some difficulty adapted to long distances. For long distances, whether overhead or underground, the bass voice is much more effective.

I perhaps should have mentioned, and I regret that Mr. Preece is not here to hear me now, that I have taken particular pains to read the discussions which were led by himself and Professor Hughes in 1878. I have read with great interest and profit what has been written on the subject; nevertheless, I would say that the number of wires worked in this country and in England are quite different; where they work ten wires we frequently work one hundred.

Mr. Preece, in one of his speeches here, which are always replete with interest and instruction, ventured to state that the "researches" of Faraday should be the electrician's Bible. I myself am ready to say, having received so much benefit from Mr. Preece's writings, that I think we could venture to add his work to the list and call it the "Electrician's New Testament." [Applause].

The gentleman who spoke last also referred to his remarks published in the journal of the Franklin Institute in 1878. I did not have the good fortune to hear those remarks, nor the pleasure of reading them at the time in the journal of the Franklin Institute. I have since read them and noted what was stated therein, and I am cordially disposed to agree with everything there stated. However, the fact that the telephone could be more easily worked over a leaky line was noted, and

verified at an earlier date than that, by Professor Bell, who used to stand on the ground with the wire only in his hand, and serve himself as a ground wire.

In conclusion, I should like to say that, while we have very many satisfactory laboratory experiments, and while I may be able to converse with San Francisco or Denver in occasional experiments, it is quite a different thing to do it every day. Because I can do it one day, I do not wish it understood that I can do it every day. Conditions vary from day to day very materially.

Professor HENRY S. CARHART. There is one word I have to say with respect to Lord Rayleigh's paper. I heard the most of that paper. I was not in at the beginning of his explanation, and therefore did not take down the formula. But Professor Rowland is entirely correct, I think, in the general drift of Lord Rayleigh's argument. The manner in which Lord Rayleigh arrived at the result of 20 miles was, of course, to take the constants of the Atlantic cable and substitute them in his formula, and also to assume a note of given pitch, and then the overtones would be cut off exactly as explained.

Mr. KOYL. I think that the conclusions which we have seen just now may be materially changed by another condition, and that is leakage. It is very easy to understand how, if we have two parallel wires, a current in one of them in one direction will start a momentary current in the opposite direction in the other. But if we have a small leak across, the leak and the induction balancing, the result is *nil*. Consequently, a cable which has a proper amount of leakage (and this expression "proper amount" is very important) may be a very successful cable. There is in this country a cable 2 miles long under water, containing seven wires, which makes a continuous length for the distance of 14 miles. That cable was put down some three or four years ago, and for a long while—I am not certain whether it is so to-day, but I know for a year or two—that cable was just as good for telephonic purposes as any 2 miles above ground in this country. There was no induction noticeable; there was nothing that was not desirable. I knew the gentleman who had charge of the cable, and he used to experiment upon it regularly. His explanation of the theory of a good cable was entirely unsatisfactory, but I think this is the reason, that the cable under water leaked and there was enough leak to prevent the induction.

Last winter I had occasion to construct an arrangement for a gentleman with which to make some delicate tests. I had a long line with a make-and-break circuit, and two telephones on a line parallel to it, at a distance, say, of 2 feet. A small current through the make-and-break circuit could be distinctly heard in the two telephones. Now, when I held the two telephones, one at each ear, the sounds, when they were equal and very low, instead of appearing to be in my ears, appeared to be directly at the back of my neck. But if I increased the effect upon the telephone at the right ear the sound would move around towards

the right ear. I then had a scale made marked with 180 degrees from ear to ear, and had a boy stand behind me. The sound in one telephone remaining unchanged, by altering it in the other telephone I could move the sound around the back of my neck. I could hear, or rather feel, the sound there, and the boy would read off the number on the scale. I could tell the difference of intensity between the two telephones in that way within something less than 10 per cent. The theoretical reasoning at the bottom of it I do not know; all I know is the facts themselves.

Adjourned to Saturday, at 10 a. m.

SIXTH DAY.

FRANKLIN INSTITUTE, *September* 13, 1884.

The Conference called to order by the president at 10 a. m.

The following preliminary report was adopted:

FIRST PRELIMINARY REPORT OF THE COMMITTEE ON ATMOSPHERIC ELECTRICITY.

The committee of the United States Electrical Conference recommends to the United States Electrical Commission that it urge the Government to take steps to secure on a large scale observations of atmospheric electricity and strength of earth currents.

That the methods of observation and report conform as far as possible to those about to be disseminated by the international bureau of telegraphic administration at Berne.

That the Government appoint a permanent committee of five electricians to cooperate with the Chief Signal Officer of the Army in organizing this service.

That this committee include, among others, the electricians of the Western Union Telegraph Company, the Bell Telephone Company, and others of wide experience.

CLEVELAND ABBE.
FRANCIS E. NIPHER.
W. H. PREECE.
JAMES ALLEN.

Professor ABBE. The next meeting of committee will be held October 1 or 2 at Baltimore, immediately after the conclusion of Sir William Thomson's first lecture, by which time further details will have been elaborated.

The following preliminary report was accepted:

THE FIRST PRELIMINARY REPORT OF THE COMMITTEE ON A UNIT OF POWER.

The committee appointed to consider the question of adoption of a unit of power (or the rate of doing work) have under examination the various practical units in America and Europe. They are based either on the foot-pound or on the kilogrammeter. Now, since the admirable system of absolute measurement called the C. G. S. system has come into general use, it is most desirable that a unit of power should be added to this system, so as to connect all mechanical and physical operations involving the expenditure of energy together in a convenient and scientific manner.

The absolute C. G. S. unit of power is the erg per second, or the work done in one second in overcoming the force of one dyne through a distance of one centimeter.

This unit being too small for practical purposes, it has been found more convenient to take ten million times its magnitude, or 10^7 ergs per second, and to call it the "watt."

This is the rate of doing work when the current of one ampere is maintained through a resistance of one ohm, and thus the work done by an electric current is brought into intimate relation with all other kinds of work.

The practical unit of power in common use is the horse-power, and is equivalent to 33,000 foot-pounds per minute. The horse-power as a unit has all the defects of an arbitrary and unscientific standard. It involves the use of coefficients, and is not connected directly with the absolute system of measurement; it differs in different parts of the world, and its name is misleading. Watt himself determined by careful experiment that a good Clydesdale horse did on the average 22,000 foot-pounds, but he added 50 per cent. to this to make his unit more in consonance with the powers of his steam engines.

Now, if the value of the present horse-power were raised 34 per cent., it would be equal to 1,000 watts, and would be connected directly with the C. G. S. system. The new horse-power would be equal to 54,293 foot-pounds per minute. Numbers expressing existing horse-powers would have to be multiplied by 0.746 to bring them to the metric horse-power, while the metric horse-powers multiplied by 1.34 would be expressed in the old horse-power.

This committee therefore recommend that the "watt" be accepted by the Commission as the unit of power, but that the change in the value of the horse-power herein proposed be reserved for further consideration.

The appended table gives the relative value of the different units now in use:

Units.	Equivalents.	
Horse-power	33,000	foot-pounds per minute.
Do	550	foot-pounds per second.
Do	75.9	kilogrammeters per second.
Do	1.01385	force de cheval.
Do	746	by 10^7 ergs per second.
Do	0.746	metric horse-power.
Do	746	watts.
Force de cheval	75	kilogrammeters per second.
Do	542.48	foot-pounds per second.
Do	0.9863	horse-power.
Do	736	watts.
Watt	0.0013405	horse-power.
Do	0.1029	kilogrammeter per second.
Do	10^7	ergs per second.
Do	0.736	foot pounds per second.
Metric horse-power	1000	watts.
Do	1.34	horse-power.

Professor ROWLAND. The report of this committee is of course incomplete, because the same committee has also in charge the consideration of the standard of light.

Captain MICHAELIS. I feel impelled to make a suggestion as an American citizen, and perhaps, also, as a member of this Conference, with regard to a matter that has been considered by this committee. I find that in naming these units very many of the great names of those connected with the advancement of science have been applied. But I have yet failed to find that a single American name has been applied to a unit, and I think we now have the opportunity. I think the new horse-power which the committee proposes to establish, namely, a thousand watts, might very properly without interfering with the established symbol, be called a "henry." We have in this country, according to

he last census, over three million horse-power in active operation, and we have besides the Falls of Niagara in reserve, which will more than double this amount in the future. We have now more horse-power in operation than any other nation in the world, and I think in giving the name to that unit our country deserves consideration.

Mr. KOYL. The addition of the name of Henry where kilo-watt would mean all that the other would mean would not be in full accordance with the system we have at present. The addition of another name would be, for example, as if some one should endeavor to give a name to kilometer and call it a franklin, or something of that kind.

Lieutenant FISKE. If we are to carry out this system entirely it is unnecessary to have a watt while we have an erg. The only question is where we shall stop.

Professor ABBE. I think the committee used the term "metric horse-power" in their report. That seems to meet the case. "Horse-power" and "metric horse-power" are very different terms.

Lieutenant MURDOCK. It seems to me we have enough horse-powers now. We have, in the first place, a mechanical horse-power changed into electrical horse-power. We frequently have to deal with French force de cheval, and in addition the metric horse power would seem to confuse the thing. I think the term kilo-watt is much preferable to any other name whatever.

Professor ROWLAND. The committee does not recommend anything with respect to it, but refers to it for further consideration. It only recommends with reference to legalizing the term "watt." But I may say here that the definitions of these units must be very carefully prepared before any laws are made upon them. If we define the term, "watt," it will, of course, depend upon the unit of resistance which is adopted. If we take 106 centimeters of mercury, it will make a difference when compared with the Rayleigh unit of 106½. We may rely on one-fourth of one per cent. variation in adopting any measurement of work upon this system. I imagine that this is the reason that the Paris Conference did not recommend this term. We already have horse-power which is based upon a number of pounds raised one foot, and if we have another unit of work which is based upon the value of the ohm which is to be legalized, the two will come into conflict to the extent of one-fourth of one per cent. I suppose the Paris Conference foresaw that, and therefore did not recommend any unit of work.

Professor GIBBS. In regard to the names, especially of the units, there are two things which are most important; one is that they shallbe universally adopted, and the other is that they shall be universally and easily understood, especially that they should not be liable to misinterpretation and misapprehension. With regard to the general adoption of this unit, I believe it has been suggested to you before that there is some little hesitation on the part of the French and on the part of the Germans. I do not know that we care particularly for a name which

is to be confined to our language. We want words in the present state of science which will be adopted by all the important nations of the earth. Of course I am unable to express any opinion as to the probability of the adoption of it by the other nations if it is adopted by the English and ourselves.

Another point strikes me in this connection, and that is this: will the watt, as it is defined, be one of the units known as the C. G. S. system of units? We shall have to say No, because it is the fundamental law of those units, which was stated when they were first announced, that all the units should be derived from the centimeter, gramme, and second by a simple process not involving multiplication by any other number, whether a power of ten or not. It is true it is perfectly easy in calculations to multiply by ten, or a thousand, or a million, without any danger of error, but it is not always so easy to remember whether or not one has to do it. The C. G. S. unit of power is called an erg per second. That cannot be altered; but this watt, as now defined, being ten million times that quantity, will not be a C. G. S. unit of power. It will be, on the other hand, a unit of power belonging to what is sometimes called the practical system of electrical units. It is the same as the volt-ampere.

It seems to me it would be an advantage if we would continue in the scientific world as we have begun, to name the electrical system of units after electricians. That is much more important in work or power than in anything else, because there is danger of misunderstanding. There is nothing in the name of this unit of power to indicate that it belongs to the electrical units. It strikes me that that is an objection, perhaps a very slight one, to the name; but I should like to have the names to be as far as possible such that one who does not hear or read of them very often, and whose memory is not very good, will be able to place them at once in the proper list. Otherwise it would be necessary to have a pocket dictionary to refer to, because the number of names threatens to become somewhat extensive.

Lieutenant FISKE. Would it not be well to adopt Lieutenant Michaelis's suggestion to have a 1,000 watt called a "henry," and not have a watt at all. That would be electrical instead of mechanical. It would also be the tenth power instead of the seventh power of ten, and therefore it would carry the electrical idea.

Professor W. H. HARKNESS. To a large extent I agree with Mr. Gibbs; perhaps in minor particulars I may differ from him. This question of units is always one which is a most significant question to the world. In the Middle Ages we find that all the trades and professions were cut up into guilds, each having its special gauge. Thus there is the watch-maker's gauge, and in the matter of wire, the Birmingham gauge, and the American gauge, the French gauge, the English gauge, and gauges without number.

Now, all these gauges are measured, without exception, by some unit of

length. It would have been much better to stick to that measure in the first place, to begin with. For my own part, I have always regretted the introduction of the French system of units as a very great misfortune. If we were only confined to electrical units, there would not be much trouble, and perhaps everybody will agree that the C. G. S. system is the best that could be adopted. As long as we use electricity in moderate quantities these units answer sufficiently well all practical purposes; but in modern developments of electricity, to which we run dynamos by steam engines of many horse-power, the case becomes somewhat different; we want larger units than the C. G. S. system of units. But just there is the difficulty. The dynamo machines, for the present at least, will be driven by steam engines, and I do not think in this connection the French units will ever be adopted by the mechanical profession in this country. In speaking of the matter, a gentleman who has a large shop said to me that—

Most unfortunately years ago we adopted the French system, and now the shop is divided, one-half running with the French units and one half with the English units; and if there were not so much money invested in machinery and appliances for the manufacture under the French system, we would have nothing to do with it whatever. We are out of pocket from it every day. The English system is much simpler.

In the measurement of electrical power developed by a steam engine we have the two directly in contact; and whether the watt is really an electrical unit seems to be an open question. Energy is no more elec tricity than it is anything else. For my part I am opposed to the whole system of names. It seems to me that some multiple of erg would be better. We have microfarads, and so on, and some such analogous name applied to the erg would perhaps be better.

Professor ROWLAND. This Conference is not to establish this unit for the whole world, although I suppose one of the principal considerations would be to be in conformity with the rest of the world, and not to set up units for ourselves; but if the word "watt" is now used, as I believe it is in England, we have that much in favor of it.

If there is nothing further to be said on this subject, I have a resolution which Professor Newcomb has given me to make, as he has departed from the city. The motion is that the Committee on the Standards of Light be requested to report a system of applying such standards in measuring the powers of electric light.

We have appointed a committee to consider the subject of the standard of light, but we did not direct the committee to devise means for the comparison of electric lights with this standard. Now, in the case of incandescent light, more light is given out in some directions than in others. It might be desirable for the committee to devise means for a uniform system for the measurement of these incandescent lights. Shall we measure them in one direction or in another, or shall we take an average all around? In the arc systems we have the light in one direction several times greater than in any other. There should be some

way of determining how these lights should be measured, so as to eliminate that factor.

There is considerable difference in the modes of measuring electrical lights and other lights. Some of the methods in use are very faulty. I think it is universally admitted that to-day the arc system has claimed very much more for its candle-power than the circumstances warrant. Indeed, it has been stated to me (and I only give this as hearsay, showing that something should be done on the subject) that there is a system of measuring the light in four directions, and adding the four results together and calling that the lighting power of the lamp. If there is no law to regulate this matter, if one company does so, all the rest will have to do so in self-defense. Therefore some legal action is needed to secure a uniform system by which we can determine whether an electric light is a thousand times greater than a candle light or not.

The motion was carried.

Professor ROWLAND. I think there are no reports of committees. Is there any miscellaneous business?

Lieutenant MURDOCK. I would like to ask for information. My recollection is that the committee upon the standards of light should make a preliminary report. I would like to ask if any such preliminary report has been offered?

Professor ROWLAND. The chairman of the committee who had this matter in charge has been called away by sickness in his family, I believe; therefore there is no preliminary report. But I should think it desirable that the report of the committee should be brought to the knowledge of electricians before it is transmitted to Congress by at least an informal notice to the electrical journals of what action is to be taken, so that the matter can be discussed in that way.

There being no more miscellaneous subjects before the Conference, I will call upon Captain O. E. Michaelis for a paper on the electrical investigation of the physical qualities of structural metals.

Captain MICHAELIS. It is only within the last few days that I came to the conclusion that this subject was a proper one for consideration in this Conference, and I have only had time, amid the multiplicity of other duties, to put my ideas together in a very crude form. I call it "A suggestion relative to the electrical investigation of the physical properties of structural metals."

In recent years the inexplicable failure, after most searching tests, of metallic construction members has been happily called "fatigue."

This quality has heretofore eluded, so far as I know, determination prior to use. As an instance of its occurrence, I submit a brief paper read before the American Society of Civil Engineers, "A peculiar phase of metallic behavior."* The metal in question was an alloy of copper and zinc, 93 and 7, in round numbers, respectively. As will be seen,

*Transactions American Society of Civil Engineers, vol. xi, December, 1882.

the strain curves of accepted and rejected lots of this metal were almost identical. Both were submitted to similar tests, searching and exhaustive, and both withstood them successfully. One, however, made cartridges that could be issued; the others, cartridges that now and then burst in the head—a fatal defect. The paper details the treatment of the metal, the shop or working test, the laboratory test, and the powder proof; further description is therefore unnecessary. The failure appeared to be due to "fatigue," whatever that may be. A visit to the rolling mill convinced me that this quality was acquired by the metal in "breaking down." The bars are annealed before rolling, and the adequacy of the annealing is determined by the color. As the bars are annealed in batches, only a general chromatic effect is produced; there must be individual variations, yet the bars are all forced through a procrustean set of rolls, which virtually says to them, "Go through you must, whether it suit you or not."

The bars that do not take kindly to the reduction, owing probably to unsuitable individual annealing, become "fatigued," and, apparently, in instances, carry this weariness through their practical life. The question is, Is this assumed affection merely sentimental, in which case it should be referred to the Society for Psychical Research; or is it due to a physical cause, and therefore a proper subject for the consideration of this Conference? I assume the validity of the latter alternative.

This physical cause must be manifested by some molecular change so slight, so elusive, that our existing system of tests is insufficient for its detection. Can the disease be diagnosed electrically? May not "fatigued" metals, in comparison with standards, develop a difference of electromotive force or offer a resistance varying from the normal? During the past year I have conversed with Professor Barker, of the University of Pennsylvania, upon this subject, and, unless I am mistaken, he agrees with me that an electrical investigation may lead to useful results. Specimens purposely abused in rolling might be tested, and if their electrical properties varied from those exhibited by standard pieces, more refined experiments might be made. The vagaries of steel are well known. Why steel hardens, especially in different degrees, under similar treatment, is still unanswered. Hidden faults too often do not make their appearance until too late for practical cure. I believe that the galvanometer will in the future enable us to reply to the question; that it will make steel our most important structural metal, a constant product, and that it will become the divining rod of the engineer in the location of defects. Here again applied science appeals to pure science for aid; the problem must be studied in the laboratory and the solution applied in the work-shop. It is a matter of vital interest not only to the Government but to the whole country. Within the past few years the Government has erected at Watertown Arsenal the first testing machine in the world, and Congress has authorized the chief of arsenals to carry out therewith programmes

of tests of structural materials submitted by the American Society of Civil Engineers.

Congress is always liberal when it can be shown that what is asked will conduce to the material welfare of the country. Were this Conference to give an expression of opinion upon the subject so briefly and crudely mentioned, and to appoint a committee to bring this opinion to bear upon the proper officials and Congress, fruitful results might ensue.

Mr. KOYL. Some time last year, while engaged in making some experiments on metals in order to ascertain their tensile strength, I found the effect called "fatigue" in certain alloys brought very strongly to my notice. A mixture of zinc and copper, ordinarily called brass, is, I think, the metal most marked in that particular. I made some investigations as to the metals which manifested this quality the most. I think that, without exception, or at least with very few exceptions, the alloys are the ones most decidedly affected; the pure metals, or metals approximately pure, are very slightly affected in this manner. A brass spring which has the elasticity and tensile strength necessary for certain purposes, on exposure to the sunlight for three or four days becomes very much like a piece of wrought iron; it has no spring at all, and will break in two without difficulty.

Not being in attendance this year at the university, I suggested to one of the gentlemen that he should investigate the matter. It seems he has not done so. But I am inclined to believe from some evidence we have that the electromotive force between copper and zinc, or the difference of potential, is the root of the whole evil. The metal is porous, and I think that the interior must be more or less moist. The slight amount of moisture in there allows an electric current to be generated, and oxygen is given off, which I think oxidizes a part of the metal, and in that manner disintegrates it, so to say. Whether this is a proper term or not, it is easily understood. It breaks up the connection between the copper and the zinc. I have not proved it, but I think that there is every reason to believe that this is the case. I will mention what is suggested to me as an explanation of the whole affair, and the cure. It is what is called deoxidized bronze. There is a method of making bronze much less porous than usual by excluding the air while under high temperature and cooling to a low temperature. The metal formed in that way has a greater tensile strength and much less electrical resistance. I think that making metal in that manner is a cure for "fatigue."

Professor HOUSTON (in the chair). If there are no other remarks on this subject, we will pass to the discussion of the third topic on the programme, which is measurement of large currents.

Mr. KEITH. I desire not to make any suggestions, but to ask some questions. I have a process which may be called an electro-metallurgical one, in which we use currents of considerable strength. The ques-

tion has arisen how we may mathematically determine and construct a galvanometer which will show the strength of the current. So far I have graded galvanometers by the deposition of lead in which there is electrolysis of no other metal, a very slight polarization, and no deposition of hydrogen, calling a deposit of 59 and some decimal grains of lead per ampere hour the constant from which I graded my galvanometer. In this case a round copper bar of $1\frac{1}{8}$ inches diameter carries the current. If a magnetic needle be suspended near this bar a deflection takes place. I have, in constructing my galvanometer, suspended this needle by a torsion spring and then graded it. The question is whether this conductor of $1\frac{1}{8}$ inches in diameter shall be considered the shaft or axis around which the magnetic currents revolve, or whether we shall consider the center of this bar as the true axis of the magnetic lines of force.

Professor FORBES. I have had a little experience in measuring currents of some thousands of amperes; and there are two methods which seem very simple. There is really no serious difficulty in the matter that I have found hitherto. One is a method, which has been explained just now by Mr. Keith, of suspending a magnetic needle over the copper bar at a considerable distance. It does not require to be a matter of a centimeter or two off, but a good many centimeters off, so that there is no difficulty about determining the distance between the needle and the center of the current; then we can calculate by the ordinary well-known formula the absolute current which is flowing, provided we know the horizontal component of terrestrial magnetism. But the other method, which I have more generally used, and which does not involve the insertion of a galvanometer into the circuit, or the determination of the horizontal component of the earth's magnetism, is simply by using means which are always at hand in any physical laboratory of measuring differences of electrical potentials between any two points. Simply take a conductor of known resistance and measure the difference of potentials between its extremities by a volt-meter. As a matter of precaution, I vary the size of this conductor. Of course, it requires considerable care as to the temperature. In my own experiments the accuracy was not required to be closer than 1 per cent. or so, and as long as the difference of temperature in that conductor was very slight between the time when I was measuring the resistance and the time when I was taking the measurements of the currents, I was perfectly satisfied. But it is perfectly easy to devise means of making an actual measurement of the temperature of the circuit at the time. I think for all practical purposes, unless we wish to go to a matter of very great refinement, we will find that method is perfectly satisfactory of measuring the difference of potential between two points the resistence between which is known.

Mr. KEITH. I have also pursued that plan. But there is this objection in making exact determinations: the longest possible stretch of

conductor which I could have in this case is 28 feet. The temperature of this conductor, an indefinite quantity, depending upon the temperature of the air, &c., would require very careful determination each time for exact measurements. And while I did not go into any mathematical calculation in regard to these, having a ready means of determining the current by the actual volt-meter method—that is, by the deposition of the metal—still, the question which suggested itself to my mind was whether taking a large conductor and a small one, the magnetic whorls, as we call them, actually may be considered as revolving around the surface of the conductor, or whether its actual center may be considered the axis of these whorls, which may pervade the mass of the conductor itself; and whether in calculating the distance of the needle we shall take into consideration its distance from the axis of the conductor, or take into consideration its distance from the surface. In other words, how is the electrical current distributed in this conductor?

Professor FORBES. As a matter of fact, the inverse distance should be integrated over the whole cross-section; that is to say, the action of any part of the current upon the needle, assuming this conductor to be a perfectly straight line of practically infinite extent, varies inversely as the distance of that part of the current from the needle. So we have to integrate $l \div r$ over the whole cross-section. I think probably for any ordinary purposes, where we are wishing to measure large currents, we may have this distance so much greater than the size of the conductor that we might very safely assume the distance from the center to the needle as the true distance. But when we want to come to more exact determinations it would be more correct to integrate over the whole cross-section of the conductor.

Probably, in the measurement of very powerful currents, it may be found very desirable to use the method which Lord Rayleigh has been investigating lately, and which other people have been talking of a great deal, and that is by the method of rotation of the plane of polarization. When we are dealing with several thousand amperes a very few turns are sufficient to give a very large rotation of the plane of polarization, either in bisulphide of carbon or Faraday's heavy glass; consequently we are enabled to make a fairly accurate determination. It is difficult ordinarily to make a determination of the turning of the plane of polarization closer than to about four minutes; but when using powerful currents we can get a rotation with a very few turns of half the circumference of a circle, and then an accurate measurement becomes easy. The attempt has been made by Lord Rayleigh and others to determine this angle absolutely, so that we shall know by the indications of the pointers, as we do by the tangent galvanometer, what is the current when we have a certain rotation of the plane of polarization. But probably, for commercial purposes, it would be better to standardize each instrument when it is made.

Lord Rayleigh has found that bisulphide of carbon is quite unsatis-

factory to work with, and is quite convinced that Faraday's heavy glass, or some other heavy glass, is not only more convenient for portable purposes, but gives far less trouble in experimental work.

Professor ROWLAND. In respect to Mr. Keith's point, I would say that the integration brings out that the effect is exactly inversely as the distance from the center of the wire. The integration, for example, through the whole earth shows that its attraction depends simply on the distance from its center. That is not the case, however, when the wire is curved around the arc of a circle. In that case the inner part of the wire carries more current than the outer, because it is a shorter distance around inside.

With regard to the measurement of strong currents, I mentioned in my address the electro-dynamometer as the proper instrument for this purpose. A dynamometer carefully made, so that its measurements are accurate, is an immensely costly affair, and only a few can possess it. Indeed, I do not suppose there are more than three or four in the world with constants which are known with any great degree of accuracy. It would require a thousand dollars or so for one to be made, and then the constants must be determined with great accuracy. I do not know that instrument makers would know how to determine the constants or anything else about the instrument. Therefore, some scientific institution must make a large electro-dynamometer, and then others may be compared with this. In that case we will have a form of instrument whose constants do not vary. These ampère meters and volt meters that depend upon the magnetization of iron I have not had any experience with, but it is my opinion that they are constantly varying. In the electro-dynamometer the force depends entirely on the dimensions of the instrument and not on the iron or anything else about it; the constants once determined, could be used forever. I have devised a simple form of electro-dynamometer, and given it to an instrument maker to make, but I do not know when I shall get it; some time, I suppose. I think it will meet these demands, but of course I cannot say much about it at present. It is not patented, and any other instrument maker can make it if he chooses.

The electro-dynamometer has this additional advantage, that it measures the integral square of the current, or, in other words, the quantity on which the energy depends; whereas the tangent galvanometer only measures the current. A current such as comes from any dynamo-electric machine is more or less irregular, as somebody has shown by putting a telephone in the circuit and finding that he could hear sounds. The tangent galvanometer does not take into account the irregularity of the current, and does not give the proper quantity for the energy of the current; but the electro-dynamometer will take into account all the irregularities, whether alternating machines or not. Therefore, we can make a series of tests on a number of machines, whether the current is

regular, alternating, or otherwise, with the same instrument, and thus eliminate one source of error.

Professor HOUSTON (in the chair). I will call on Lieutenant Jewell to open the discussion on the application of electricity to military and mining engineering.

Lieutenant JEWELL. I desire to submit a few remarks on a question which, perhaps, is not of general interest to electricians, but which is of the utmost importance when considered from a naval point of view; and I do so with the hope that among the inventors and makers of dynamo-electric machines present, there may be some who will give us the desired solution. I refer to the question of a dynamo-machine for the purposes of naval warfare.

Two kinds of lighting are demanded for vessels of war. First, powerful lights must be supplied for use in offensive operations against land works, for protection against the approach of torpedo-boats, for use in communicating between distant points at sea, and, what is less important, perhaps, the navigation of difficult channels at night. The demand for such a light is the most important, because it will add to the fighting capacity of the vessel, and it is as a fighting machine that the dynamo-machine presents itself. The other kind of lighting is required for the internal lighting of the vessel, the decks, holds, and engine-room. This demand is almost of equal importance with the other when we take into consideration the fact that upon the sound condition of the health of the crew depends so much the efficiency of the ship as an engine of warfare.

The sanitary condition of the lower decks of vessels of war is notoriously bad, and necessarily so while we are compelled to adhere to the present methods of lighting by means of foul-smelling oils, which are constantly introducing into the atmosphere below decks carbonic acid and other vitiating gases.

In all naval vessels which hitherto have been fitted with electric lights in this country, it has been found necessary to introduce two types of machines to fulfill the purposes I have enumerated. For the purposes of search-lights two Gramme dynamos, connected directly to an engine which gives both of them the same speed, are employed. Each of these dynamos supplies current for a single lamp, and furnishes, with a potential difference of about 50 volts at the terminals and a current of 40 amperes, a light of about 20,000 candles, the lamp being of a peculiar construction, so that the light is delivered in the desired direction.

The two dynamos are, however, connected to a switch arrangment by which they can be coupled in parallel on either light, and thus supply a light of more than double power. For internal lighting the usual incandescent machine is employed, belted to the driving machine. Thus we have three dynamo machines to do the required work, two of which must be driven at the same time whether we wish to make use of the lights furnished by both or not.

Nearly two years ago I suggested the construction of a machine which would serve both these demands of the naval service. Within the present year such machines have been constructed in England, and are now being supplied to the English navy; and it is my purpose to outline briefly the character such a machine should possess.

For search-light purposes an electro-motive force at the terminals of 60 or 65 volts with a current of from 75 to 100 amperes is as great as we can conveniently handle. If we get more current than this the projectors must be made inconveniently large.

A compound-wound dynamo giving an approximately constant E. M. F. at terminals of 60 volts, no matter what the resistance in the external circuit, is the dynamo that we require, and it could also be used for interior lighting when the search-light is not required.

With a machine so wound there would be no difficulty in "striking" the arc, which is apt to occur with a simple shunt-wound machine, nor would it be subject to the violent shocks, both mechanical and electrical, due to breaking the arc in a series dynamo. To economize space, which is a most important consideration on board ship, the dynamo must be driven direct without the intervention of belts. To accomplish direct driving the speed of the dynamo must be kept low in order to insure reliability and durability of the engine; 500 revolutions per minute would seem to be the greatest speed permissible. The utmost attention should be paid to the mechanical design and finish of the dynamo, in order to insure long life, and all similar parts should be absolutely interchangeable.

Each dynamo should have its own engine, so as to be entirely independent of the other. The cases would be few in which two search-lights would be required at once, and I am firmly convinced that one powerful light is better than two inferior ones. In fact, I am almost inclined to believe that one search-light is as good as half a dozen under almost all circumstances.

To sum up, then, what we want is a compound-wound dynamo driven direct, capable of supplying 200 to 250 16-candle incandescent lamps, or a single arc lamp of 20,000 to 25,000 candle power and a constant potential at terminals of 60 volts, when driven at a speed of 500 revolutions. Such a dynamo should not absorb more than 25 to 30 horse-power, although the question of economy is in my opinion not the most important one. Two such machines, with their driving engines, would form part of the outfit of a vessel of war; one or both of them could be used for the search-lights, should occasion require, while under ordinary circumstances one alone would serve for interior lighting.

Of the form of lamp required for a search-light, I will only say that the lamp must be regulated by hand and have proper appliances for adjusting the arc in the focus of the reflector. Such lamps are not difficult of manufacture. The lamp should be so inclined that the incandescent face of the positive carbon may be presented at the most favor-

able angle. Short arcs are desirable, *i. e.*, arcs only sufficiently long to permit the whole of the incandescent face of the positive carbon to be presented without being obscured by the negative carbon. As to projectors, I doubt whether any great movement can be made in the projector of Colonel Mangin, so largely used abroad, though I have no doubt the manufacture of that projector might be profitably undertaken in this country.

With regard to the possible profit to be derived from the construction of such a machine as I have outlined, I have only to say that the application of such a machine is not confined to vessels of war. Space is as valuable on our coastwise and inland steamers as it is in the Navy; and it is of the greatest importance to them also to have a dynamo for driving their incandescent lamps of such construction that any number of the lamps may be turned out without influencing the rest. Besides, a compound-wound dyanmo would be capable of supplying a number of small arc lamps which could be used at convenient points when discharging cargo or loading at night.

Professor HOUSTON (in the chair). The Conference will proceed to the discussion of lightning protection.

Professor ROWLAND. As this is an important question, especially in some of the Western States, I will say a few words.

In order to protect buildings from lightning we must have a space into which the lightning cannot come, and have the house situated in that space. What sort of a space do we know in electrical science into which electricity cannot enter from the outside? It is a closed space—I mean a space inclosed by a very good conducting body. All the lightning in the world might play around a hollow copper globe and it would not affect in the slightest degree anything inside the globe; but the walls of the vessel need not be solid metal. Of course, if solid, it is all the better; but if it is made of a net-work of very good conducting material it would protect the inside from lightning strokes. A spark striking on one side of such wire cage would find it easier to go around through the wire of the cage to the other side than it would to go through the center. This is Maxwell's idea, with reference to protection of houses from lightning, viz, to inclose the house in a rough cage of conducting material. Suppose, for instance, this box is the house, and suppose we start from the roof and run a rod diagonally to each corner and thence down to the earth. We thus make a rough cage. Of course there are openings on the sides; and if we wished to make a better protection we could put rods down the sides wherever we wished. Now, there is ground underneath the house, and the lightning might, by jumping across the center, find a good conductor through the middle of the house and go down to the earth in that way. How do we prevent that? By running the lightning-rods clear across underneath the house. Then the lightning would find it easier to go around the house than to jump across, even if there were a good conductor

through the middle. A house inclosed in a cage of that sort would be perfectly protected, even if it were a powder magazine, or anything of that sort. Of course, in the case of petroleum storage resevoirs, where fumes are given off, there would be danger then, as the stroke might ignite the fumes of the petroleum. That would not be the case of a powder magazine. The protection in that case could be made perfect.

It is not necessary to have lightning-rods insulated. Indeed the question is, can we insulate a lightning-rod? We may insulate it for a small potential, but lightning coming from a mile or two to strike a house is not going to pay any attention to such an insulator; we may just as well nail the lightning-rod directly to the house as far as that goes.

The idea of having the lightning-rods inclose the bottom as well as the sides of the house is very important, because we do not know, and we have no right to assume, that the earth is a good conductor. We are perfectly certain if the earth forms a good conductor that then the lightning could go down at the sides into the earth. By inclosing the house in a case both below and above we obviate all that difficulty, and it makes no difference whether the earth is a good conductor or not.

I am glad of this public opportunity to say something with regard to a peculiar form of lightning-rod; it is in reference to a form of a rod shaped like the letter U. I think the idea is that the lightning strikes on one side, and that it goes down and has inertia and flies up again. The company which advocated this idea had the impudence to bring a lawsuit against a scientific man who said it was a humbug. A company of course can make a great deal of trouble to one man; but when there is such a gross humbug as that around, one would like to undergo the danger of a lawsuit. There is nothing scientific about it; it will endanger life in any house in which it is placed.

Mr. SCOTT. I would like to ask whether a building constructed of iron would not be completely protected from lightning?

Professor ROWLAND. Yes, if it has a floor of iron too. If a gas-pipe came up into the center the lightning might find it easier to go across to the pipe than to go around. But if we made a floor of iron the lightning would find it easier to go around than across to the pipe. It must be an entirely inclosed house.

Mr. SCOTT. Then would not a petroleum tank entirely constructed of iron with an iron bottom be the safest inclosure possible for petroleum?

Professor ROWLAND. The peculiarity of that is that the fumes of petroleum are all the time coming out from the cracks. The whole outside is probably covered with petroleum. I suppose also the ground is saturated with petroleum. The petroleum as far as the inside goes would be perfectly safe.

Lieutenant FISKE. I would like to ask how far lightning obeys the ordinary law of currents, whether it takes the path of least resistance

or not. Do high potentials always do that? In general across a narrow space the resistance is greater than going around by the iron, and the question is, to what extent does the lightning obey the law of circuits?

Professor ROWLAND. I would like to say one word more with respect to petroleum. In the case of the tank you have a mixture of the petroleum vapor and air which probably would explode. Unless the tank was a very good conductor there might be also a little spark in the interior, not enough to hurt a man in there; but the smallest spark inside the tank would cause an explosion. I am not certain whether the iron of the tank is a good enough conductor to prevent every trace of spark in the interior. Indeed, suppose we had a tank with a cover upon it. That is supposed to be a closed vessel, yet the lightning would have to pass from top to bottom between the cover and the tank, and perhaps a little spark would take place in the interior; and possibly in going from one of the plates of the iron tank to the other it may find some resistance and jump over some small plate in the interior of the tank. It would be a most difficult thing to protect.

With regard to that other question, lightning in the air, of course, does not obey Ohm's law; it is entirely a discontinuous anomaly. It is like the breaking of a metal. A piece of metal is supposed to break at a certain strain; but it does not always break then; it pulls out in strings or something of that sort. One cannot measure the distance and say the lightning is going to jump across that distance.

Professor HOUSTON. The question of lightning protection is one of very great importance. I do not think it is so necessary here to discuss the principles of the subject as it is to consider the best means of disseminating what we do know. The ideas of the average lightning-rod man are of an exceeding curious character; and if some well-devised scheme could be planned whereby the public could be informed as to the exact state of scientific knowledge in the subject of lightning protection it would be of very great importance.

Speaking of Professor Maxwell's system, I think it is an admirable one to protect a house, but not so good a system to protect a neighbor's house. The main requisite in lightning-rods, as we all know, is to have the cross-section of the rod sufficiently great to safely carry off the heaviest bolt received in that vicinity. That would probably necessitate a weight to the bar of copper of 6 ounces to the running foot. If the general public were informed of this fact and were to compare the rods in actual use with those that are recommended by the Lightning-Rod Conference, they would, I think, be very much surprised.

The lightning-rod should be attached directly to the building, and should not be insulated from it. This is the conclusion reached after the most exhaustive examination of all the cases on record. Of the greatest importance of all is the point least borne in mind, and that is, that the lightning-rods should be thoroughly grounded. A rod simply

put into the ground, even if it is carried into moist earth, is almost insulated.

As to the condition of the point of the rod, considerable attention has been given to that recently. Of course almost any rod that could be used would act as a point. It is, however, preferable to have the rod pointed, and it is a matter of considerable importance to see that the point of the rod is not so sharp that the bolt will fuse the point, lest the fused particles fall upon the roof and produce fire.

As to the shape of the rod, of course we are all very well convinced that it is mass, and not surface, that is needed. Here, then, is a thing upon which the general public should be informed. It should be made clear to them that flat and twisted rods are bad for actual use, since they increase the liability to oxidization.

As to the space protected by the lightning-rods there is considerable doubt in the minds of practical electricians. Some say it is twice and some that it is 1.75 times the conical space which would be produced by allowing the rod to fall and move around the horizontal plane of the building which is to be protected. There seems to be considerable doubt as to how high the lightning-rod should extend above the top of the house to afford protection to the roof; and if the roof is very much extended, how many rods should be placed upon it to protect the building.

There are one or two points of a rather curious character that I want to allude to in passing. I noticed in a scientific paper a very curious statement made with reference to the geographical distribution of the injurious thunder-storms. The paper made the statement that on the basis of the destructive fires produced by lightning as returned by the Board of Fire Underwriters in this country, that most of the loss from destructive lightning storms in this country took place in a section, as I remember it now, east of the Mississippi and north of the Ohio. I think meteorological students will agree with me that this is improbable. The law of the distribution of thunder-storms would not cover that area. I do not want to get into a lawsuit with any lightning-rod man, but it seems to me to be a very pertinent inquiry as to what system of lightning-rod protection there is in that section of country. There can be no doubt that the lightning-rod system in common use in many parts of this country is a positive menace to the buildings. I have recently had submitted to me for examination lightning-rod points which I understand are in general use, that you can crush between your fingers. Rods are used of entirely insufficient cross-section to carry the stroke. They are very generally insufficiently grounded. Rods are not infrequently made that look like masses of solid copper, but are in reality masses of cast iron, wrapped around with an exceedingly thin foil of copper. Such rods are full of joints; and we are by no means assured that the joints are good.

Professor Rowland was talking about a curious feature in lightning-

rods, illustrating the ignorance of those erecting them. I think I can match that. There was a pamphlet issued lately as to the true method of lightning protection. Any one who would put such a system on a house deserves to be hung. The system I allude to consists in erecting a vertical rod pointed at each end and carefully insulated from the house. The idea is that the lightning strikes at one point and is dissipated at the other. The originator says it is pretty much on the induction principle; it does not pass off but as the points allow it to.

The principal thing we want to establish is not how the building shall be protected, for we know that already, but to let the public get this information, so that they may know how to treat the lightning-rod man.

Professor ROWLAND. As to that point I can say that, as far as experience goes, we usually find that people like to be humbugged, and would a great deal rather believe a lightning-rod man than a scientific man. But the time for adjournment has come, and it would be well to express our thanks to the officers of the Franklin Institute and others for their kindnesses.

Mr. ALEXANDER GRAHAM BELL. I move that a hearty vote of thanks be offered to our distinguished vice-president, Sir William Thomson. and to the other distinguished electricians from abroad who have honored us with their presence, and assisted us in our deliberations.

Seconded and carried.

Captain MICHAELIS. I offer the following resolutions:

Resolved, That the thanks of this Conference are due, and are herewith tendered to the Franklin Institute of Philadelphia, for courtesies extended and for facilities furnished, which have largely aided the rapid advancement of the business of the Conference, and contributed to the increased enjoyment of its members.

Resolved further, That the Conference thanks its Chairman, Secretary, and other members of the Commission for the able, impartial, and agreeable manner in which they have administered its affairs.

The resolutions being seconded were unanimously adopted.

The PRESIDENT. The question has been raised as to whether we shall hold any further meetings of this Conference. It seems to me that the occasion for it will almost have passed away. The occasion of the Conference has been the Electrical Exposition of the Franklin Institute and the presence in this city of the members of the British and American associations, so that altogether we have had a body of distinguished men, such as we could seldom get together. But some have expressed the feeling that there must be something further for the Conference to do. If any member of the Conference has anything to suggest on this point it can be presented now. Is it desired that any further meetings should be held?

Professor ABBE. I only express the opinion of a great many here when I say that we all agree to what has been suggested. We need very much a meeting of this kind frequently in order to exchange ideas. I am not an electrician, either professionally or practically, and it hardly becomes me to say what they need, but I can see how lively and how

agreeable these meetings are. I very much hope that some means may be taken to call us together occasionally, perhaps by ourselves, perhaps by a committee.

Professor ROWLAND. I do not suppose that we could be called together officially, except it be done within a short time. Although the law is very broad under which this meeting of the Conference has been held, we could not construe the law to make this Conference into an electrical society; that would be impossible. If within a short time a meeting is desired, I suppose that it may be called under the present law.

Mr. KEITH. Pertinent to that subject I would like to say a few words. Early last spring, this exhibition being contemplated, and it being desired that the electricians should assemble themselves together in an organized body, some gentlemen in New York issued circulars to various electricians and professional men throughout the country with the stated object of forming a society. Such a society was formed last May, and the proceedings of that meeting were published, and have been circulated to some considerable extent. The membership has risen to 283. This society will meet here in Philadelphia on the 7th, 8th, and 9th of October. A number of members of the Conference are also members of this society. It is called "The American Institute of Electrical Engineers."

I shall take the liberty of sending to members of this Conference copies of the rules and minutes of the meetings held so far, so that they may become informed as to the standing of this society, and if it seems desirable to any one that is not now a member, he will be gladly welcomed into the society. We want to make it a useful institution, and make it large and financially strong as well as intellectually strong; if not financially strong it certainly will not be intellectually, because finances support intelligence.

Professor PREECE. I should not allow this time to pass without being allowed to express on the part of Sir William Thomson and my confrères on the other side of the water who have come over here, their deep acknowledgment to you and the Commission and the Conference for the kindness that has been shown to us. It has been a great comfort to me to come to this great country where one can speak one's own language. We have been in the habit, sir, of attending meetings in France and other parts of the world where we had to speak or try to speak in another language. And the first comfort we have here is that we are among our brothers. The attention that we have received from the very first moment that we stepped upon these shores has been nothing but true courtesy—almost brotherly love.

I know that all of us will return feeling deeply the sentiment that has been extended to us, and the uniform kindness that we have received. I cannot too strongly urge the necessity of repeating such meetings as this. It is wonderful how much business is smoothed when we come to know personally the men with whom we transact business.

It is wonderful to note the increased pleasure we have in hearing a paper when we are personally acquainted with the man who has written it. It is marvelous how all little professional jealousies are entirely removed when the conference is to take place among friends. I have noticed here, once or twice, a slight tendency to consider criticism as something personal and unnecessary, and the debate has at times been a little warm, but personal meetings of this kind entirely eradicate that feeling. I therefore hope that you will organize more meetings, and if you do I will cross the Atlantic again and join in your deliberations. [Applause.]

Professor ROWLAND. Some of the members of the Conference think that they have received more from their English friends than they have given. Indeed, I think that this is the opinion of all, that the success of the Conference has depended in a very great measure upon the presence of our distinguished guests. [Applause.]

Resolved, That this Conference deem it important to have one more final meeting, and adjourns subject to the call of the chairman.

Seconded and carried.

Professor ROWLAND. I will say with reference to this motion that before making any call of this meeting it will be necessary to send out circulars to see how many can attend, and where the place of meeting could most conveniently be held. I do not think anything of a success could be attained otherwise.

Lieutenant FISK. I would like to suggest that it would be easier to get a great many together in New York than in any other place.

Mr. KOYL. I am requested by Captain Michaelis to bring this motion before the house. It bears upon the question of his paper on the structural qualities of metals. The subject is of such importance that knowledge is desirable. It so happens that he agrees with the explanation that I brought forward in reference to that subject.

Resolved, That this Conference deems the electrical investigation of the physical qualities of structural metals a matter of great importance to the community at large.

Resolved, That in view of the governmental action already taken relative to the tests of structural materials and the active interest evinced by the prominent engineering societies of the country in the matter, a committee of seven be appointed by the Chair to investigate the subject, and to bring it before the proper officials and Congress.

Resolved, That this matter be referred to the said committee to be brought up at the next meeting of the Conference or otherwise, as may seem best to the Commission.

Professor NEWCOMB. I second that motion. I think it is an admirable idea, and a work which will probably result in considerable benefit to science and manufactures.

Professor GIBBS. We have had a very interesting subject introduced to us by Captain Michaelis. It seems to me that a subject of so much novelty requires a little more thought before we can take such definite action as this. He has brought forward many points to reflect upon. Some of us may have something useful concerning them in the future.

In the infancy of a work like this it seems like putting a heavy duty upon the chairman to ask him to appoint a committee which should carry to any successful result an investigation of this kind, or suggest to Congress what could be done in a matter of so novel a character.

Lieutenant FISK. I like the suggestion. I was in the Washington navy-yard in 1878, and Lieutenant Stone distinguished himself in this direction, and drew up a plan more or less complete for extended experiments. He did start some experiments, but shortly afterward he was ordered to sea, and those experiments were not resumed. It seems to me that magnetic tests form a way to approach the subject as electrical tests. I move that the methods be electrical and magnetic instead of electrical only.

Mr. KEITH. I would state that magnetic tests as to the homogeniety of steel are constantly made in the various steel-making establishments of the country.

Captain MICHAELIS. I have been struck by the remarks of Professor Gibbs. I think that they are very forcible. Therefore, I move that this resolution be referred to the Electrical Commission, either to be brought out before the contemplated meeting of the Conference, or for such other action as the Commission may in its own time think proper.

The question being on the motion of Captain Michaelis, it was adopted.

On motion of Professor Newcomb the Conference then adjourned.

REPORTS.

REPORT OF COMMITTEE ON ATMOSPHERIC ELECTRICITY.

SIGNAL OFFICE, *May* 18, 1885.

Professor ABBE:

At your request I make the following brief statement of work now carried on by the United States Signal Service in the study of atmospheric electricity.

Through the generous co-operation of Johns Hopkins and Harvard Universities, and of Professor Rowland, at Baltimore, and Professor Trowbridge, at Cambridge, the Signal Service has maintained for some time observation stations at these points. The work of observation has been carried on by the men belonging to the Signal Corps of the United States Army under the advice and direction of Professors Rowland and Trowbridge. At Baltimore a continuous registration by means of a Mascart electrometer has been maintained. As a collector the water-dropping apparatus has been used and photographic registration has been employed.

Some important improvements in Mascart's apparatus have been made, most of which will be found described in Signal Service Notes No. XVII, by Park Morrill, private, Signal Corps, U. S. A., who is doing the work under the direction of Professor Rowland. This report also contains a brief discussion of the observations on electric potential considered in connection with other meteorological phenomena.

No continuous photographic record has been maintained at Cambridge, but regular observations at stated periods have been kept up for a year, and much valuable work has been done in the study of form of apparatus. Several important modifications of well-known forms of electrometers have been devised by Professor Trowbridge, and their use has given most gratifying and promising results; experiments have also been made here by Privates McAdie and McRae, under the direction of Professor Trowbridge, in the use of kites for measuring the potential of high points in the air.

Preparations are now making for a continuation and extension of this work at the office of the Chief Signal Officer in Washington. It will be attempted first to make a thorough study of the instrumental parts of the problem, to the end that such forms may be selected as will be reliable in their indications and convenient in use; this to be followed by a consideration of the subject of exposure, and it is hoped that in the near future we shall be prepared to undertake a systematic course of observations at a number of properly distributed points.

In addition the service has already successfully inaugurated a system of thunderstorm reports through a special corps of about 2,000 observers. Another body of about 1,200 observers report upon tornadoes.

No steps have as yet been taken in the direction of the collection of information from telegraph and telephone companies concerning ground currents or other electrical disturbances which are likely to come under the notice of their employés. It has been thought best to defer such an undertaking until preliminary experiments should determine what might be done and how the work may best be carrried on in order that the collected information may be of real value.

Respectfully,

T. C. MENDENHALL,
Professor and Assistant, Signal Office.

REPORT OF COMMITTEE ON STANDARD OF LIGHT.

Experiments on the production of a standard of light have been conducted by Professor Pickering, Professor Cross, and Professor Trowbridge, since the meeting of the United States Electrical Commission in Philadelphia, September, 1884.

Professor Pickering and Professor Cross have made experiments upon the suitability of using incandescent platinum wire for determining a standard of light. For this purpose, instead of using the point of solidification as proposed by the Paris Conference, they have adopted the suggestion of W. Siemens and have endeavored to ascertain whether there is a suitable constancy in the melting point with a current of known strength. Many series of experiments have been made, but many more must be made before any conclusions can be reached in regard to the results of this method. Professor Trowbridge, with the aid of Mr. Edison, who has kindly made the necessary apparatus for him, has endeavored to fuse a standard upon the energy radiated from an incandescent carbon filament, this energy being measured at a fixed distance by a modification of Professor Langley's bolometer. A fine strip of steel is placed at the center of the Edison carbon loop in an Edison lamp and the heat received is measured by a Wheatstone bridge arrangement. The assumption is made that the light given off by the carbon filament when it is made incandescent by a current of known strength is the same when the heat radiated is the same. This arrangement substitutes a measure of radiant energy for the eye. Although it is not so sensitive as the eye, yet it is freer from the errors of different observers arising from difficulties of distinguishing colors than the purely optical method. These experiments are still in progress. The committee cannot hope to conclude their labors for at least a year.

JOHN TROWBRIDGE,
Chairman Committee on Standard of Light.

CAMBRIDGE, *April* 10, 1885.

SIGNAL OFFICE, *May* 15, 1885.

Professor H. A. ROWLAND,
President of the United States Electrical Commission:

DEAR SIR: The committee on atmospheric electricity, appointed in August, 1884, by the United States National Convention of Electricians, after making a preliminary report at that meeting, adjourned to meet in Baltimore early in October.

There were present at the second meeting only Messrs. Preece, Abbe, and Rowland, with Sir William Thomson and Lord Rayleigh as invited members.

It had been hoped that by this time we should have received further details as to the progress of international electrical matters in Europe, and especially such instructions for the observers of atmospheric electricity and ground currents as the International Commission might have agreed upon.

Unfortunately it was found that Mr. Preece, as a member of the committee of the International Commission, was obliged to report that such instructions had not been completed and issued. Our committee, therefore, considered that, while awaiting the action of the International Committee, and in order to avoid issuing independent instructions that might differ from theirs, it would be best to recognize the fact that the whole subject is still in its early experimental stage and that detailed instructions for general use would even yet be premature. They also recognized the great importance of preliminary studies into methods and apparatus at present being conducted by the Signal Service through the co-operation of Harvard and Johns Hopkins Universities.

The committee therefore concluded to recommend—

(1) That no general instructions be formulated for the present.

(2) That the studies under Professors Rowland and Trowbridge be diligently prosecuted.

(3) That the results of these latter be, if practicable, made available in the deliberations of the International Committee as a basis for the instructions which it proposes to issue.

(4) That when such instructions be received from that committee they be adopted so far as practicable by the electricians in the United States.

In connection with the above the committee expressed its hope that some competent electrician should be put in charge of the extensive work that had been intrusted to the Chief Signal Officer of the Army.

No meetings of the committee have been held since the preceding, nor can I find that any publication has been made of the deliberations of the corresponding committee of the International Electrical Congress.

In conclusion, I am happy to be able to say that the wish above expressed by the committee has already been realized by the appointment of Professor T. C. Mendenhall as electrician in the Signal Service, whose work in this department will be supervised by him. His report appended herewith shows the present condition of the work.

I have only to add the request that you will add his name to our committee.

Very respectfully, yours,

CLEVELAND ABBE,
Chairman Committee on Atmospheric Electricity,
Professor and Assistant, Signal Office.

COMMITTEE ON BUREAU OF STANDARDS.

In accordance with the resolution of the conference, the following committee has been named to represent the conference before Congress in relation to the establishment of a Bureau charged with the duty of examining and verifying instruments for electrical and other physical measurements: Profs. Simon Newcomb, Monroe B. Snyder, William A. Rogers, Cleveland Abbe, and J. E. Hilgard.

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National Conference of Electricians, Philadelphia, 1884
Report of the electrical conference

Engineering

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